The authors would like to thank both anonymous referees for their valuable comments and suggestions, which have helped improving 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 spent on reviewing our manuscript and for his/her positive and insightful comments that have helped improving the quality of the manuscript. We have revised 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 compensate 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 2nd 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 have expanded our discussion of the potential effects of changes in denitrification on the nitrogen cycle on longer timescales by adding a detailed statement in section 4.2.2 that summarizes the key points exposed in the discussion above (please see the added statement highlighted in red from line 5, p18 to line 9, p19 of the revised manuscript).

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 $O_2 >$ 20mmol/m^3. The 2nd pathway occurs at low O_2 (< 5mmol/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 < 5mmol/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. 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 have added a short paragraph in section 4.2.2 where we discuss the potential changes in N_2O production and consumption terms following the key arguments detailed above. More specifically, we have added the following statement:

"The increase in the Arabian Sea denitrification should also lead to an increase in the N_2O production. This could not be tested in the present study, as N_2O is not represented in our model. Indeed, as denitrification leads to both production (under suboxic conditions) and consumption (under anoxic conditions) of N_2O, 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. However, we speculate that significant monsoon intensification has the potential to lead to an important enhancement of N_2O

production because of enhanced nitrification. Indeed, nitrification is predicted to increase by up to 62% in response to a 50% increase in wind stress while Arabian Sea data suggests nitrification to be the dominant pathway of N_2O production outside of the OMZ and a major contributor, together with denitrification, to its production inside the OMZ, (Bange et al, 2001).}"(please see lines 10-19, page 19 of the revised manuscript).

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 have added the following two statements in section 3.1 and 3.2 to explain the strong control of the SWM perturbation over the NPP and OMZ annual mean responses, respectively:

In section 3.1, we have added:

"Two factors explain the strong control of the SWM perturbation over the NPP annual mean response. First, the biological production during the SWM dominates the annual production (explains more than 40% of the annual levels while NEM productivity contributes by less than 33%). Second, summer productivity is more

sensitive to wind changes as it is directly driven by wind-induced upwelling. In contrast, NEM productivity is driven by wintertime cooling and convection. Hence, NEM 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)."(please see 3.1, pages 11-12 of the revised manuscript).

In section 3.2, we have added:

"This can be partially explained by the larger summer productivity and its larger sensitivity to wind changes leading to stronger perturbation of the O2 demand. Additionally, the deepening (by up to 25m) of the wintertime mixed layer that result from NEM intensification enhances the ventilation of the northern and northeastern Arabian Sea, thus compensating the mild increase in O2 consumption that result from enhanced winter productivity". (please see 3.2, page 12 of the revised manuscript).

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

Done.

Anonymous Referee #2

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The authors used a ROMS, which was coupled to an NPZD model to study impacts of changing monsoon winds on the OMZ and the marine nitrogen cycle in the Arabian Sea. The results indicate that changes in the summer monsoon winds exert the main control on productivity, the OMZ and finally the marine nitrogen cycle. Intensification of the summer monsoon winds increases the productivity, expands the OMZ at depth, and increases denitrification, while an enhanced intrusion of oxygen-enriched surface water weakens the intensity of the upper OMZ at water-depth between 100 and 200 m. Since there are indications that the Indian summer monsoon intensifies in response to global warming, the topic addressed within the manuscript is of great relevance. The manuscript is, moreover, well-written. However, the presented model results and parameterizations of important processes deviate from conclusions drawn from field data. This in addition to some other aspects needs clarification before publication of the manuscript can be recommended.

We are thankful to the reviewer #2 for the time spent on reviewing our manuscript and for his/her valuable comments that have made the manuscript stronger. Following the reviewer suggestion, we have added model comparisons with field data to the revised the manuscript to further support our main results. Moreover, we have added a couple of clarifications as requested by the reviewer. Please see below our responses to specific comments.

1) As stated in the abstract the main conclusion is as follows: 'We show that the Arabian Sea productivity increases and its OMZ expands and deepens in response to monsoon wind intensification. These responses are dominated by the perturbation of the summer monsoon wind, whereas the changes in the winter monsoon wind play a secondary role'. Here it should be mentioned explicitly that winds are generally weak and winter cooling drives productivity during the winter monsoon (e.g. Madhupratap et al. 1996). In its present form it is misleading because it could imply that wind mixing is a dominant factor because it was selected to run the sensitivity experiment.

We agree with the reviewer that winter winds are generally weak and that winter cooling and convection drives winter bloom. We have made this more explicit in the revised manuscript by adding the following statement: *"In contrast, NEM productivity is driven by wintertime cooling and convection."* (see page 11, line 11 of the revised manuscript). Additionally, we now better explain the mechanisms through which the perturbation of the summer monsoon controls the OMZ annual mean response (please also see our response to comment #3 by reviewer#1).

This assumption would furthermore suggest that model results show that the summer monsoon is more important for the productivity as the winter monsoon. The discussion of various pale- oceanographic studies shows that warming increases wind speeds, expands the OMZ and increases denitrification. This, furthermore supports the impression that the summer monsoon is the main driver, and the winter monsoon of lower importance. This was not studied in the model and it should also be considered that these paleoceanographic results were obtained by comparing glacial and interglacial periods. During the Holocene a weakening of the summer monsoon strength seems to be accompanied by an intensification the OMZ (see e.g. Rixen et al. 2014) suggesting that ventilation plays a more important role than implied by the model output

We have improved our discussion of the mechanisms that lead to stronger control by the summer monsoon perturbation (please see our response to comment #3 by reviewer#1). We would also like to point out that our finding that the summer monsoon driven productivity exceeds that of the winter monsoon is also supported by several observations (e.g., Dickson et al, 2001). Otherwise, we agree that past ventilation changes may have played an important role in modulating the variations in the Arabian Sea OMZ and denitrification as suggested by some previous studies (Pichevin et al, 2007, Boning & Bard, 2009) and already acknowledged in our manuscript (see section 4.2.3 of the manuscript). While our study highlights the strong link between monsoon variations and OMZ fluctuations, it does not rule out a potential contribution from changes in large-scale ventilation. With our current model setup we cannot however test such a hypothesis as this would require using global simulations with a realistic representation of the Arabian Sea OMZ as stated in the manuscript (see section 4.2.3, lines 15-17, page 20).

2) The occurrence of the secondary nitrite maximum is generally assumed to indicate active denitrification in the water column of the Arabian Sea (see Naqvi et al. 1991, 1998 and more *recently Bulow et al., 2010, Gaye et al. 2013). The secondary nitrite maximum occurs a water depth between 100 and 400 m which implies that denitrification is absence or at least of minor importance in the deeper part of the OMZ. The model results show exactly the opposite as summarized in the abstract: 'The increased productivity and deepening of the OMZ also lead to a strong intensification of denitrification at depth, resulting in a substantial amplification of fixed nitrogen depletion in the Arabian Sea'. This needs to be clarified as well as the ignored N-fixation as pointed out by reviewer #1.*

We do not agree with the reviewer statement in that our modeled denitrification profile disagrees with observations. Indeed, our control simulation also shows maximum denitrification between 100 and 400m (black curve in Fig. 7b of the manuscript). For example the rate of simulated denitrification in the control run below 400m is at least a factor 7 smaller than at 200m. Our results are therefore consistent with observations made by studies cited by the reviewer (e.g., Bulow et al., 2010, Gaye et al. 2013). The deepening of denitrification referred to in the statement cited by the reviewer concerns the 50% increased wind perturbation simulation. We do not expect the model subjected to such a relatively extreme perturbation to stay close to observations made under present day forcing.

3) The parameterization of the carbon export into the deep sea should be described in more detail. Since sinking speeds and respiration rates are provided I assume that a model similar to those introduced by Banse (1990) was used. The considered sinking speeds of 1 and 10 m per day are an order of magnitude lower as those derived from sediment trap studies (see e.g. Berelson, 2001). Please clarify.

The detail of the representation of the carbon export in the model is given in section 2.1, lines 18-22, page 4. Following the lead of Gruber et al (2006), particle sinking is represented explicitly using 2 detritus classes that can also be advected laterally: a class of large and fast sinking particles (10m d^{-1}) and another class of small and slow sinking particles (1m d^{-1}). We also specify the remineralization rates used for the large (0.01 d^{-1}) and small detritus (0.03 d^{-1}). We do not use representations of export based on the Martin equation where the particle flux is set to decrease exponentially with depth such the ones referred to by the reviewer (and described in Banse 1990 or Berelson, 2001). Instead, the flux attenuation with depth emerges from the decomposition of organic matter as it sinks.

We would like to point out that it is the ratio of sinking speed to decomposition rate (corresponding to a remineralization lengthscale) that controls the attenuation of export fluxes in our model. While sinking speeds used in the model can be lower than some sediment trap estimates by up to one order of magnitude as correctly mentioned by the reviewer, the decomposition rates used in the model are also proportionally weaker than in those studies (e.g., ~ 0.2 to 0.3 d^{-1} in Banse 1990, Deep Sea Research). Therefore, despite differences in sinking speed and decomposition rates, the remineralization lengthscales in our model (1000m and 33m for large and small detritus, respectively) are comparable to those implied in some previous studies. This is supported by the reasonable agreement of our simulated export fluxes with the sediment trap observations from the US JGOFS Arabian Sea expedition (see the new Figure A6 in appendix A of the revised manuscript and our response to comment #4 below).

4) Among others satellite-derived chlorophyll concentrations were used to validate the model, which to my understanding do not agree well to model outputs. (The months given in Fig. 2 bottom need to be corrected). However, satellite data especially during the summer monsoon are problematic but there are a number of sediment trap data from the Arabian Sea (see e.g. Honjo et al. 1997 and Lee et al. 1997). Considering the importance of carbon export model data should be compared to sediment trap data to make the main conclusions convincing.

We agree with the reviewer that the modeled chlorophyll-a does not agree well with the observations in certain areas, especially off the coast of Somalia as already acknowledged in the submitted manuscript (lines 9-10, page 8). However, the fidelity of the model north of 10°N is in line (if not better) with most of state of the art models (e.g., Resplandy et al, 2012). This is also supported by the relatively high correlations and comparable variances between simulated and observed surface chlorophyll-a distributions evidenced in the Taylor diagrams (Fig 4).

Yet, we do agree with reviewer #2 comment that satellite chlorophyll data is not enough to evaluate the biological model and we thank him/her for his/her suggestion to include more field data in the model validation. Following the reviewer suggestion, we invested time to enhance our model evaluation by adding comparisons with field data from the US JGOFS Arabian Sea Process Study (1995). This consists in: i) ^{14}C primary productivity ii) export fluxes at 100m estimated using ^{234}Th removal rates and iii) export fluxes estimated from sediment trap data at 500m above the seafloor to avoid including resuspension fluxes as advised in previous works (e.g., Gardener 1992). Fluxes were measured essentially during the year 1995 at 5 sites (M1, M2, M3, M4 and M5) along a transect extending from the Coast of Oman to the central Arabian Sea. Because of the relatively limited number of individual in-situ observations of biological productivity available in this dataset (only 5 measurements at each site), we also used satellite-based productivity estimates obtained using two different algorithms: the Vertically Generalized Production Model (VGPM) (Behrenfeld and Falkowski, 1997a) and the Carbon Based Production Model (CBPM) (Westberry et al., 2008) using data from two sensors (SeaWiFS and MODIS). The results of these comparisons are presented in Fig A5 and Fig A6 shown in the appendix of the revised manuscript.

This comparison shows that the model correctly simulates a decrease in productivity and export fluxes as the distance to the coast increases (Fig A5 and Fig A6). The model, however, substantially underestimates the measured primary productivity in all 5 stations. Some of this mismatch may be due to the fact that the in-situ productivity estimates are all coming from one individual year (1995) and based on only 5 independent measurements at each site (Lee et al, 1998). Given the importance of both mesoscale and interannual variability, the in-situ estimates may therefore not be representative of the long-term climatological conditions simulated by the model. Indeed, a better agreement is obtained between the modeled productivity and estimates based on satellite observations that have a more extensive temporal coverage (Fig A5). We further contrasted the simulated export fluxes at 100m to estimates from Lee et al (1998) at the 5 stations (Fig A6). Our modeled export fluxes generally overestimate the ^{234}Th-based estimates but remain comparable in magnitude with these observations. Furthermore, the model reproduces quite accurately the observed offshore gradient in export. It is worth highlighting however that similarly to in-situ measured productivity, these export fluxes are based on 4 independent measurements at each site only, all from the same year. This may induce biases in these estimates due to contamination by mesoscale and interannual variability. We finally compared the modeled export fluxes in the deep ocean (500m above the seafloor) to sediment trap data at the same 5 sites (Fig A6). The comparison shows a good agreement between the model and the observations at all stations. It is worth noting that these deep export flux estimates can be considered as more robust than those at 100m as they are based on a larger number of independent measurements (20-40 measurements at each site).

In conclusion, despite some discrepancies, our modeled fluxes show a reasonable agreement with both field data and satellite observations. Following reviewer's suggestion, we have included a description of these new comparisons in section 2.3 of the revised manuscript (lines 7-28, page 9). We have also corrected the typo in the name of summer months in Fig 2 pointed out by the reviewer.

5) Considering the overall importance of the selected topic, which will probably attract a wider readership, I recommend to avoid Taylor diagrams and use simple xy scatter plots. They are clear and easy to interpret. Please include also data from the deeper part of the OMZ in the data / model comparison.

We prefer the Taylor diagrams over xy plots because the former provide a more quantitative and condensed synthesis of model skill. As each dot on the Taylor diagrams represents an independent comparison between the model and the data, replacing the two diagrams with xy plots would require 18 figures! Additionally, because of the large number of individual observations used in this comparison (resulting from the high-resolution of the satellite products and the large number of observations available in World Ocean Database), the xy plots may be visually difficult to read and compare. Therefore, we decided to keep the Taylor diagrams as they are widely used for model evaluation and model skill assessments in climate and environmental sciences. Please also note that Fig 4b include observations sampled down to 1000m (grey filled circles).

6) Moel et al. 2009 is missing in the reference list.

Corrected. Thank you!