Answer to Referee #2

The authors would like to thank anonymous referee #2 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 #2

Received and published: 13 June 2017

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 will make our manuscript stronger. Following the reviewer suggestion, we will add model comparisons with field data to the revised the manuscript to further support our main results. Moreover, we will add 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 will make this even more explicit in the revised manuscript. We will also explain better 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

In response to this comment, we will improve our discussion of the mechanisms that lead to stronger control by the summer monsoon perturbation (please see our response to previous comment). 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 submitted manuscript (see section 4.2.3, page 18).

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 submitted 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 these 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 3 in 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 10ºN 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 (a figure from Lee et al. (1998) indicating the location of the sediment trap moorings is included at the end of this document). 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 the following figures.

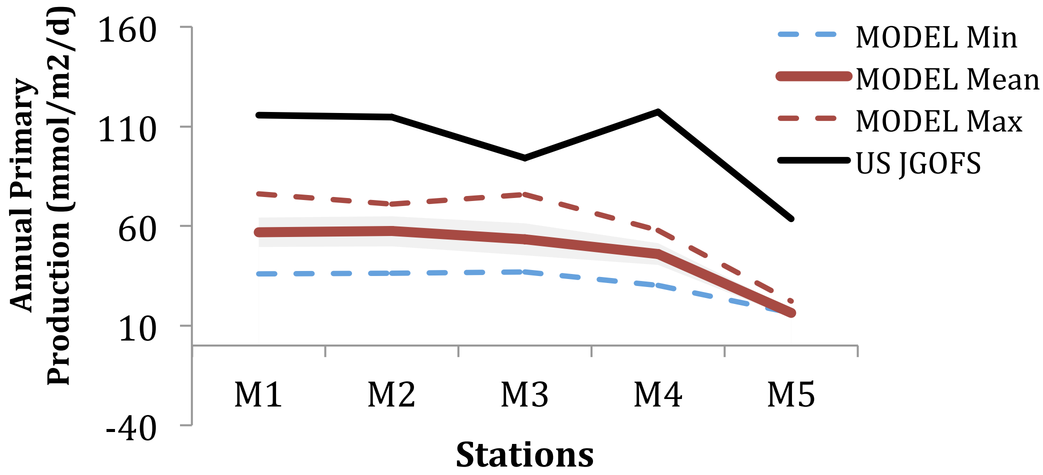

Figure 1: Annual-mean primary production fluxes at 5 mooring stations (M1-M5) as estimated from in-situ observations (black) and simulated in the model (red). The dashed lines refer to modeled maximum (red) and minimum (blue) annual production. The shading indicates the ± 1 standard deviation from the model mean.

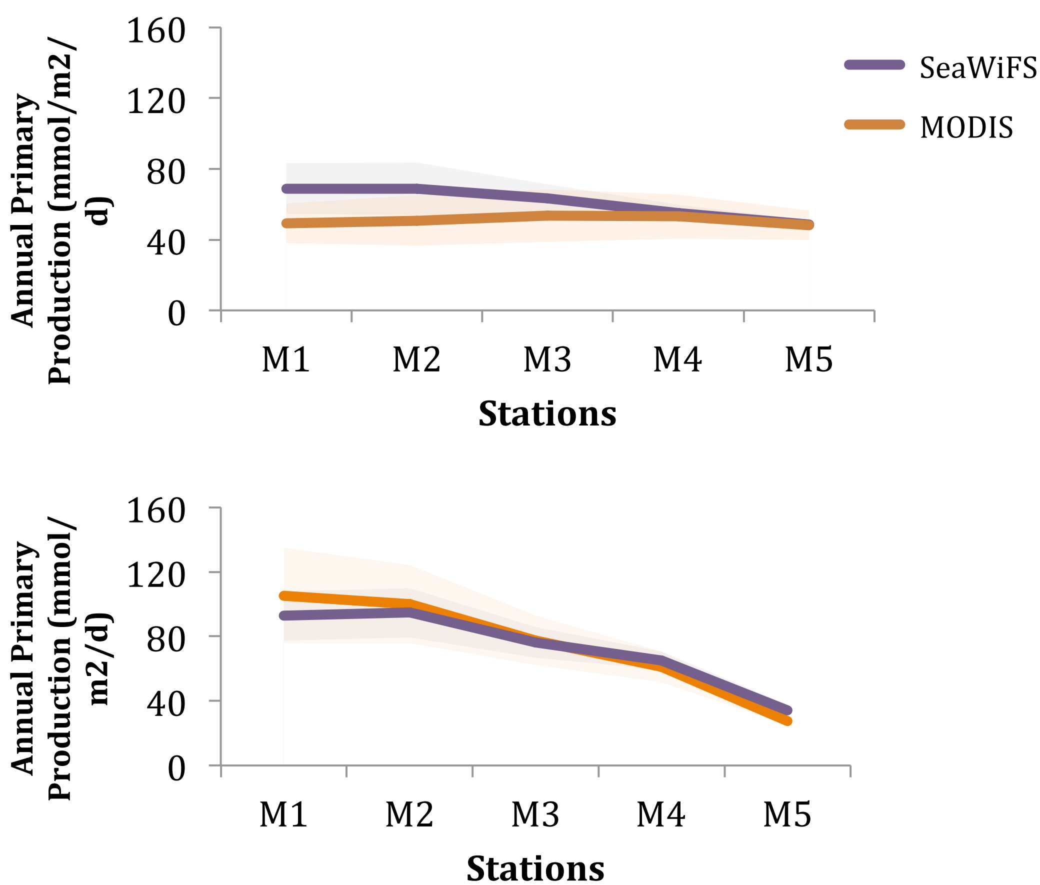

Figure 2: Satellite-based estimates of annual-mean primary production fluxes at the 5 mooring stations (M1-M5) using the CBPM (top) and VGPM (bottom) algorithms and the SeaWiFS and MODIS sensors. The shading indicates the ± 1 standard deviation from the mean.

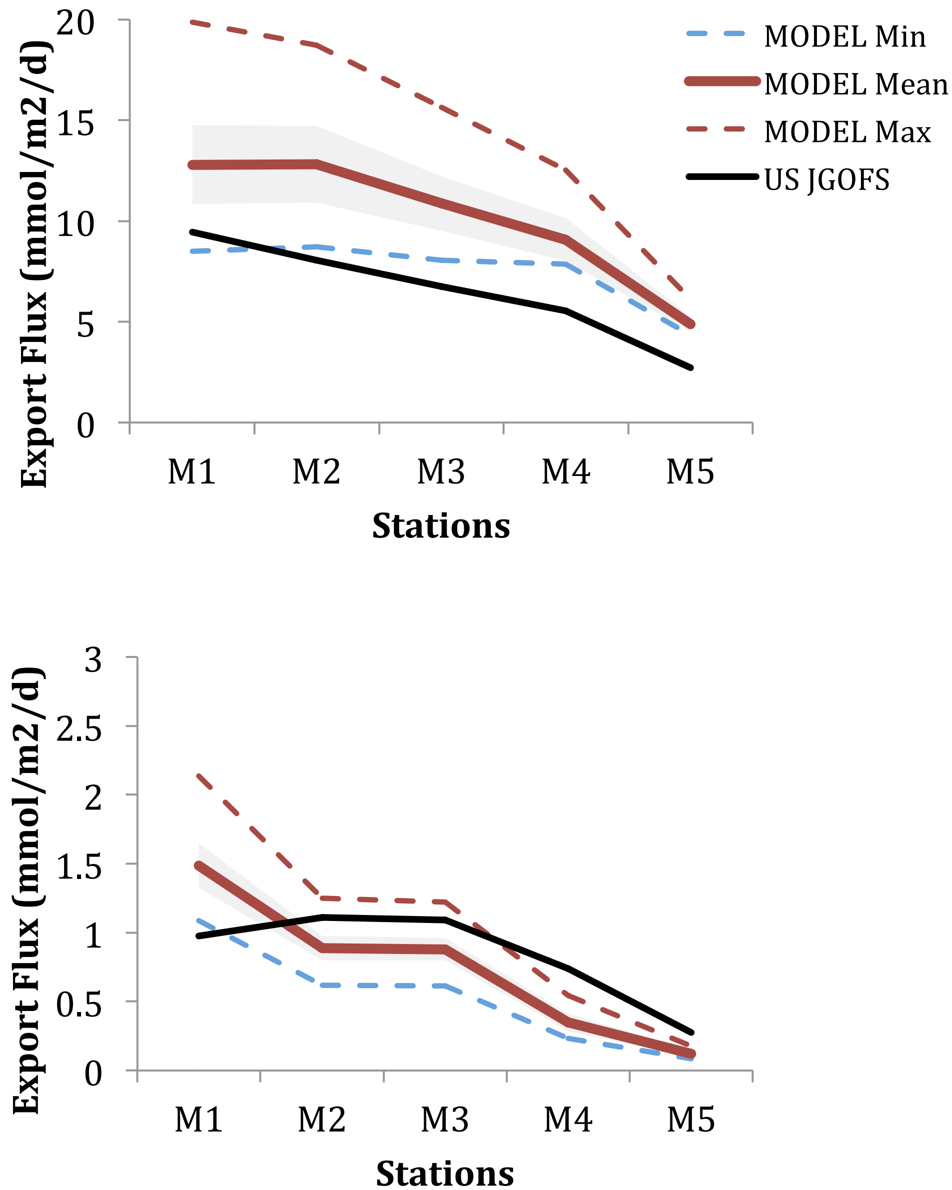

Figure 3: Annual-mean export flux at 5 mooring stations (M1-M5) as estimated from in-situ observations (black) and simulated in the model (red) at 100m (top) and 500m above the seafloor (bottom). The dashed lines refer to modeled maximum (red) and minimum (blue) annual export fluxes. The shading indicates the ± 1 standard deviation from the model mean.

This comparison shows that the model correctly simulates a decrease in productivity and export fluxes as the distance to the coast increases (Figures 1 and 3). 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, by further comparing the simulated productivity to satellite-based estimates at the 5 stations, we find generally a better agreement (Figure 2). We further contrasted the simulated export fluxes at 100m to estimates from Lee et al (1998) at the 5 stations (Figure 3). 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 (Figure 3). 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 will include the 3 figures presented above together with a description of these comparisons in the revised manuscript. We will also correct the typo in the months given in Fig 2 of the submitted manuscript.

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 (submitted manuscript) include observations sampled down to 1000m (grey filled circles).

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

Thank you, we will correct this.

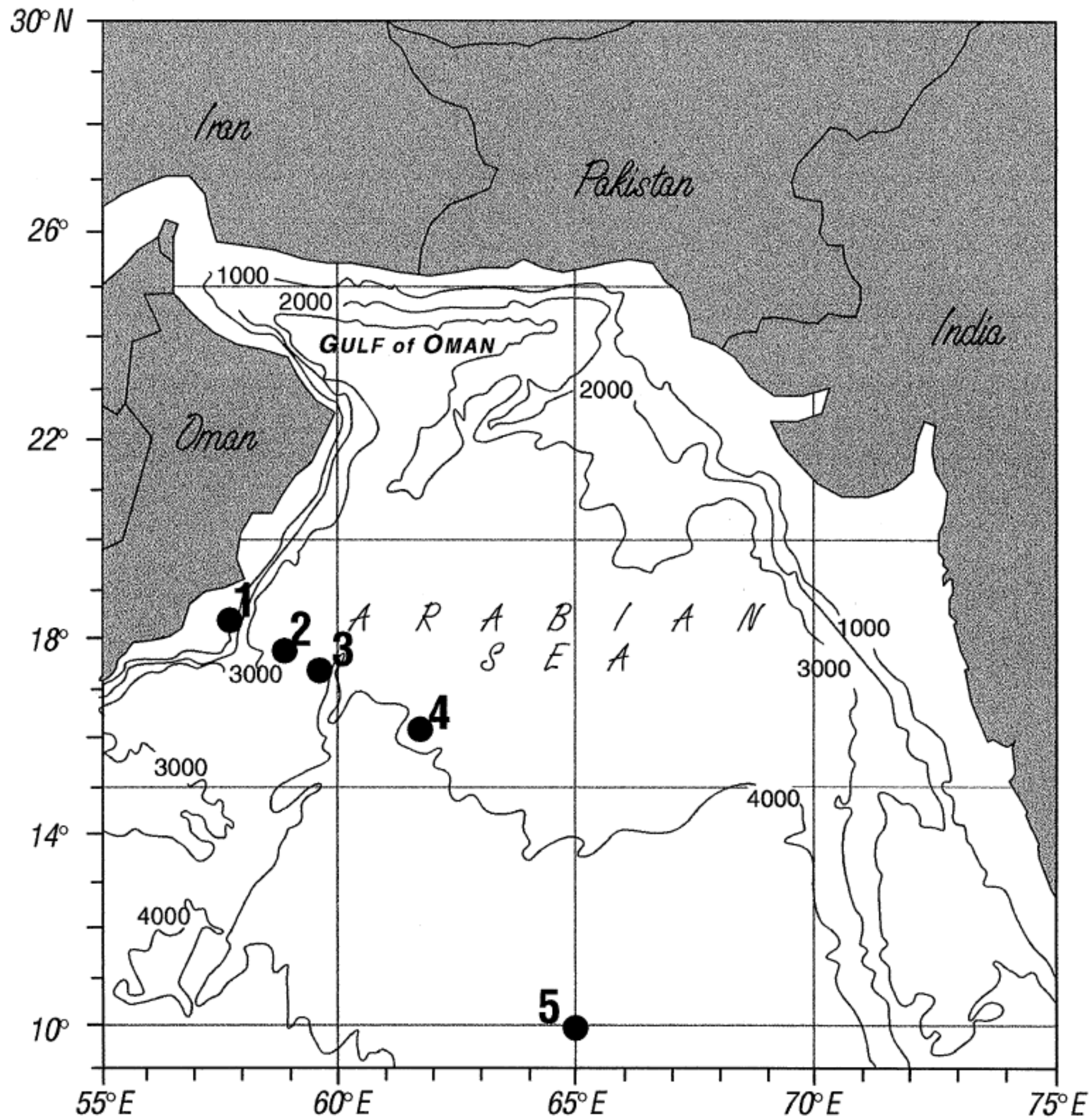

Fig. 1. Map showing location of sediment-trap moorings 1–5.

Figure 1 from Lee et al, (1998, Deep Sea Research II) indicating the location of sediment traps M1 to M5.