Answers for Reviewer 1:

The manuscript describes results of a newly developed biogeochemical model of the nitrogen cycle (BioBUS) that has been implemented into a regional sigma-coordinate model of the Namibian upwelling region. The manuscript consists mainly of a comparison of model results with observations or observational estimates of biogeochemical tracer distributions. This comparison is very detailed and appears objective. Overall, the model seems to describe many features of the Benguela upwelling system much better than other models do, although this is not firmly demonstrated by the authors. The few examples where a comparison to earlier (box) model results is given, the results are very similar.

<u>General comment:</u>

I value the paper as a fine example of a model validation. However, the paper neither demonstrates significant improvement compared to earlier model results, not does it address a clear scientific question. The main scientific result is the quantitative assessment of the lateral export of nitrogen out of the upwelling region into the open subtropical South Atlantic. I am unsure as to how new this really is. Presumably, this transport could be diagnosed from observational data bases and an estimate of the Ekman transport (e.g. Williams and Follows, DSR 1998).
Overall, I think that the manuscript is more a model description (though some details like the formulation of "burial" still need to be described in the manuscript) than a scientific biogeochemistry paper. Because of the lack of scientific questions (and answers) in the relatively lengthy manuscript, I cannot recommend publication in Biogeosciences but would recommend publishing this paper (after revisions) in a journal like Geophysical Model Development. I am not sure whether one can easily transfer papers from one EGU journal to another, but this would certainly be a very good opportunity to do so.

Answer:
As suggested by the two reviewers, we split the submitted paper in two different papers:
-One revised paper for Biogeosciences on the nitrogen transfers in the Benguela upwelling system with clear scientific questions.
-Another one with the model description, model/data comparisons, and sensitivity analysis on key processes and consequences for the nitrogen fluxes for Journal of Marine Systems. The description of the BioEBUS model and the model/data comparisons in this paper come from the version of our paper previously submitted to Biogeosciences and has taken into account all comments of the reviewer.
A technical documentation will be written later and submitted to Geophysical Model Development as recommended by the reviewer with an extended description and validation of the coupled model.

As explained above, the model/data comparison is now part of another paper for Journal of Marine Systems. Thanks to this comparison, we demonstrated that the coupled ROMS/BioEBUS model is able to represent many features of the Benguela Upwelling System (BUS) allowing us to use this model to investigate scientific questions on the nitrogen cycle.

This revised version of our paper for Biogeosciences points out now challenging scientific questions and associated answers, while the previously submitted one was more confused as noticed by the reviewer. Now, three scientific questions (see below) are clearly identified in the introduction of the paper and answers given in the result section.

1/ Nitrogen offshore export:
We estimated the total nitrogen (N) offshore export at 10°E off Walvis Bay domain and the contribution of mesoscale activity (23%). Thus, this activity plays a significant role in supplying the subtropical gyre in nutrients. N lateral export out of the BUS into the open subtropical South Atlantic cannot be only explained by the Ekman transport. To our knowledge, this is the first estimation of the mesoscale influence on the N offshore export from the BUS. We also show now the induced mesoscale circulation compared to the mean circulation (Figure 6 of the revised paper) for the first time in the Namibian upwelling.
Moreover, our results suggest that total N offshore export from the BUS contributes to 33% of the new primary production estimated for the South Atlantic Subtropical Gyre, so a significant N source sustaining primary production in the open ocean. No estimations have been made so far in the South Atlantic Ocean.

2/ Nitrogen losses by denitrification and anammox
As compared with the data we have off Namibia, our coupled physical/biogeochemical model gives satisfying estimates of denitrification and anammox processes, and is the first 3-D realistic configuration able to estimate these N losses due to denitrification and anammox processes in EBUS and associated OMZ. We show as well that these N losses off Namibia have no significant effects on the N offshore export (in the first 50 meter-depth, at 10°E) from the Namibian upwelling system to the South Atlantic Subtropical Gyre.

3/ N_2O emissions
In the last question, we show that N_2O emissions off Namibia are significant as compared to the other EBUS. Indeed, this small domain represents 1.2% of the EBUS in term of surface, however its N_2O outgassing contributes to 4.4% of the total EBUS emissions. In terms of emissions per unit area, the Walvis Bay area emits between 2 and 5 times more N_2O than other coastal upwelling areas. So, the Walvis Bay area represents an important N_2O outgassing as compared to its regional extension and to other coastal upwelling regions.

Individual comments:

1) Question:
The model is a new configuration (the "Namibian configuration") of the ROMS model. The physical model is evaluated against hydrographic observations of T and S. It is not clear whether model has reached a seasonally cycling steady state, yet. On p.3548, lines 25-27 may suggest that there is still some substantial temporal drift in the model results. To convince the reader (and me) it would be good to show a time series of some properties like upwelling transport, $NO_3/O_2/N_2O$ concentrations.
Answer:
In the revised paper, a physical spin-up is performed over 7 years (Y1-Y7), and then the coupled physical/biogeochemical model is run for 12 years (Y8-Y19): 4 years for the physical/biogeochemical spin-up (Y8-Y11) and 8 years (Y12-Y19) for the analysis of the model outputs. As can be seen on the time series of averaged volume kinetic energy, salinity, and nitrates, oxygen, nitrous oxide, and total nitrogen concentrations (Fig. 1 below), the model needs a few years to reach a seasonally cycling steady state, for the physical simulation as well as the coupled simulation. From Y12 to Y19, the coupled model has reached a stable

state, with interannual variability due to non linear processes which generate mesoscale activity.

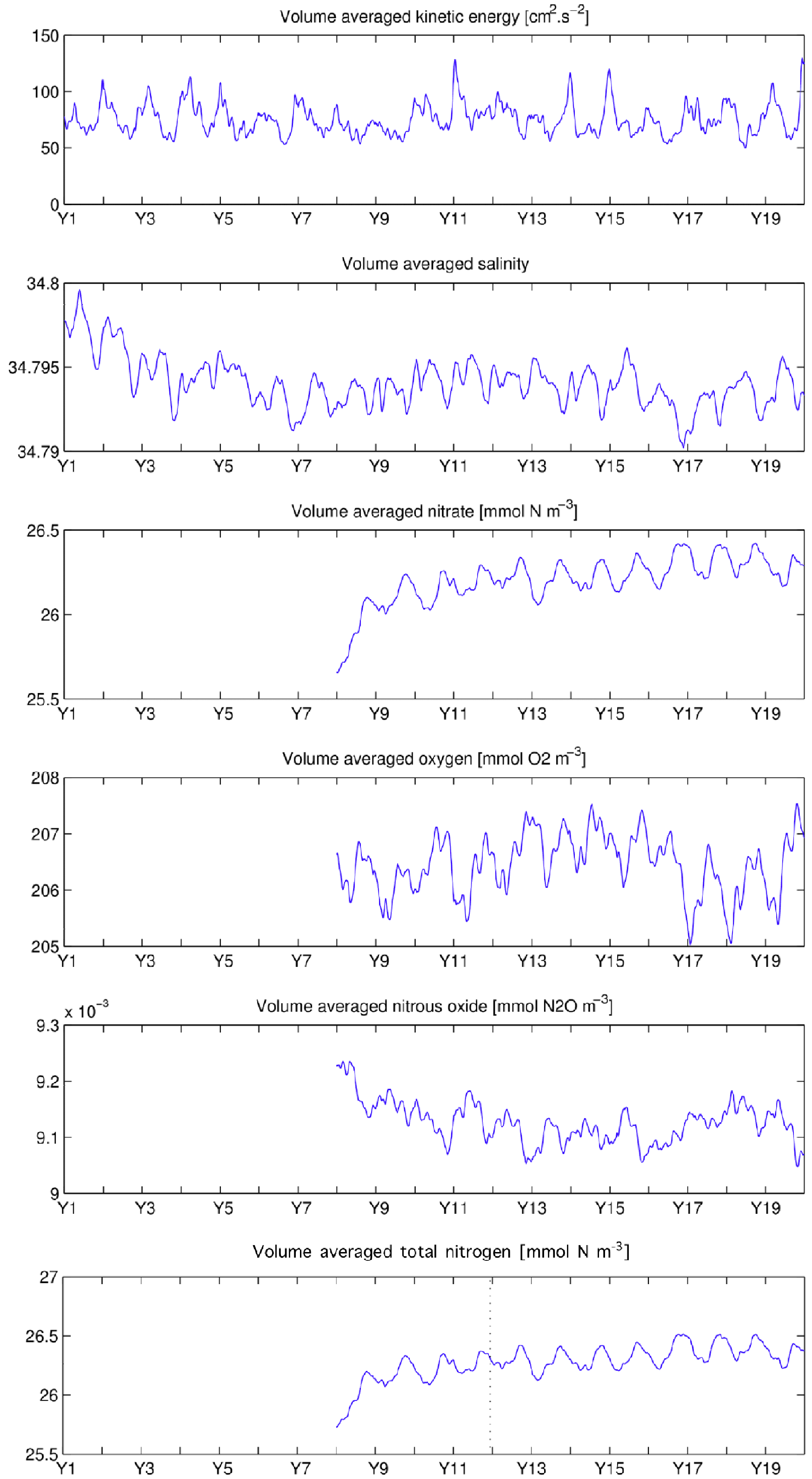

Figure 1. Time series of volume averaged kinetic energy, salinity, and nitrate, oxygen, nitrous oxide and total nitrogen concentrations for the 19-year Namibia simulation.

2) Question:
Units for O_2 concentrations should be changed to umol/l (or umol/kg or mmol/m^3). Ml/l is difficult to assess in combination with all the other N and C fluxes that are given in molar units.
Answer:
We agree and changed the ml/l units for O_2 concentrations for mmol/m^3 units. We chose the ml/l units for comparisons with previous published works. We now specify the O_2 concentrations also in ml/l units in brackets when it is necessary for these comparisons.

3) Questions:
The treatment of the sediment and the way "sequestration" is computed do not become clear. Equations in the Appendix seem to be valid for the water column only. Does remineralisation of detritus stop in the sediment? Is there a sediment layer below the last grid box of the water column? Otherwise, sediment material would be still subject to advection, wouldn't it?
Answer:
In the model, a cumulative layer exists in which detritus or sinking particles are stored. We used the same approach as Koné et al. (2005) and other models applied in eastern boundary upwelling systems. Within this cumulative layer, the detritus cannot be advected; PON just accumulates on the floor artificially in the model, without further interaction with the overlying waters.

4) Question:
p. 3540, line 26/27. N_2 fixation is not necessarily restricted to the ocean-atmosphere interface. There is enough N_2 gas resolved in sea water everywhere.
Answer:
We agree and made this change in the revised paper version (line 91/92).

5) Question:
p 3542, l.4 & 6. "suboxic..." "During these anoxic events" not clear what is meant here. Are you referring to the same suboxic=anoxic events?
Answer:
This sentence was not precise enough.
We made the following change, lines 125-131 of the revised paper:
"In this OMZ, suboxic concentrations below 25 mmol O_2 m^{-3} (or ~ 0.5 ml O_2 l^{-1}) are encountered in Walvis Bay (Monteiro et al., 2006, 2008), and even below the detection level during some periods of the year. During these extreme events, in addition to the respiratory barrier that affects zooplankton and fish (Ekau et al., 2010), sulfur emissions can occur with subsequent impacts on the mortality of commercial species (benthic communities such as demersal fish, lobster and shellfish; Lavik et al., 2008)."

6) Question:
p3542 , l.14. "alleviate". This does not seem to make sense.
Answer:
We agree and replace by "could potentially mitigate", line 133 of the revised paper.

7) Question:
p. 3545, l22-24. Is there no N_2O consumption at very low O_2 concentrations? If so, this could perhaps be stated explicitly.
Answer:

In the revised version of the paper (lines 196-198), we changed the parameterization of Nevison et al. (2003) for the parameterization of Suntharalingham et al. (2000, 2012) which takes into account the N_2O produced during the denitrification process. We do not take explicitly into account the consumption of N_2O during the second step of the denitrification as the parameterization of N_2O production is based on O_2 concentrations. Moreover, N_2O consumption occurs at very low O_2 concentrations (< 1-2 mmol O_2 m^{-3}) (Gruber, 2004). In our simulations, there are few oxygen concentrations below 1-2 mmol O_2 m^{-3}, and over few pixels (Fig. 2 below). Thus, we will not affect our estimations by adding a N_2O consumption term or changing our N_2O parameterization with a decreasing function from very low O_2 concentrations up to 0 mmolO_2 m^{-3} as in Jin and Gruber (2003),

Jin and Gruber, 2003. Offsetting the radiative benefit of ocean iron fertilization by enhancing N_2O emissions. Geophys. Res. Letters, Vol 30, NO. 24, 2249, doi:10.1029/2003GL018458.

Gruber, N., 2004. The dynamics of the marine nitrogen cycle and its influence on atmospheric CO_2 variations, in The ocean carbon cycle and climate, edited by M. Follows and T. Oguz, Kluwer Academic Publishers, pp97-148.

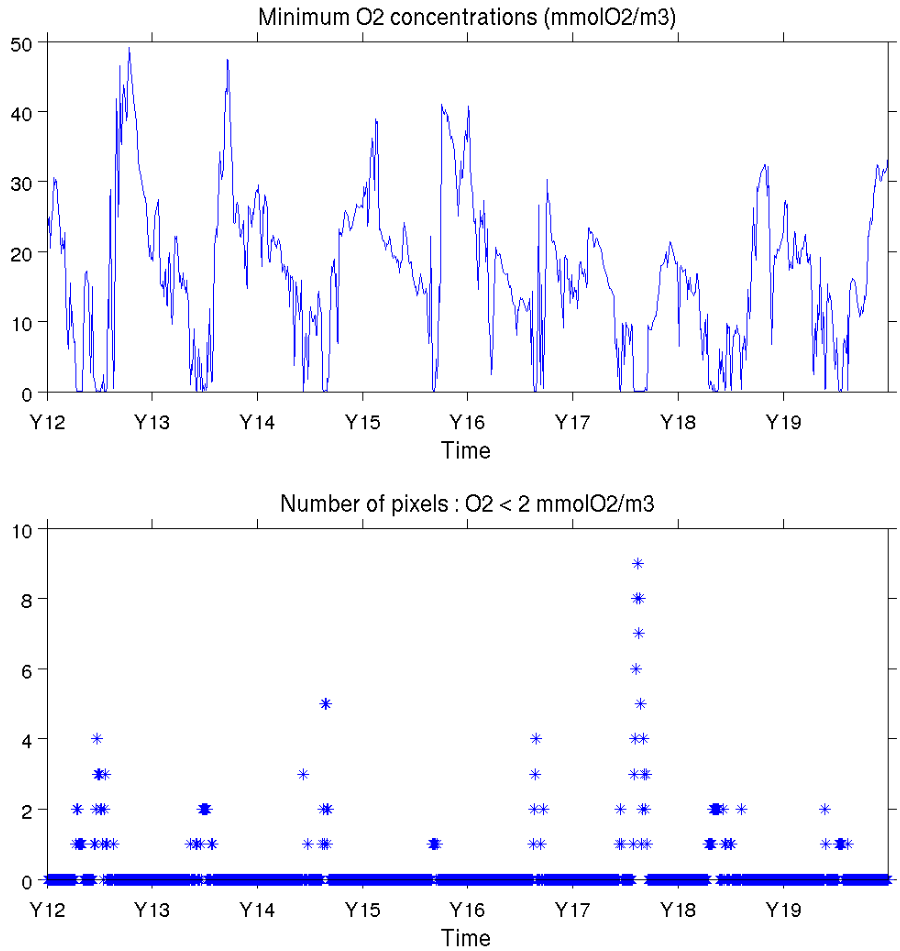

Figure 2. Minimum oxygen concentrations (top) in mmol O_2 m^{-3}, and number of pixels where oxygen concentrations are below 2 mmol O_2 m^{-3} (bottom) for the whole analyzed domain.

8) Question:

p.3546., l.12 ""is better simulated as compared to data". What is meant here? Better than what?

Answer:

"With these modifications, the general spatial distribution of the different types of plankton from the coast to the open ocean in the BUS is better simulated as compared to data" has been changed to: "With these modifications, the general spatial distribution of the different types of plankton from the coast to the open ocean in the BUS gets closer to the data", in the validation paper for the Journal of Marine Systems.

9) Question:
p.3547, l.21, Are maximum growth rates given for a temperature of 0 degrees Celsius?
Answer:
Yes, in the submitted manuscript, a_{P_i} represents the maximum growth rate of phytoplankton for T=0°C (the maximum growth rate: $J_{max} = a_{P_i} b^{cT} = a_{P_i}$). This sensitivity study is now in the other paper for Journal of Marine Systems, and has been removed from the present revised manuscript for Biogeosciences.

10) Question
p.3550, section 4. Please specify whether the model is interpolated onto the observed data or vice versa. Also, it would be good to say whether all data are assumed statistically independent from each other and whether there is weighting applied to account for the different volume of different model grid boxes.
Answer:
For the figure, no interpolation between the data and the model grids has been made. For the statistics, the model is interpolated onto the observed data. All data are assumed statistically independent from each other.
To clarify this point, we added "For the statistics, the model is interpolated onto the observed data locations.", lines 235-236 of the revised manuscript for Biogeosciences.

11) Question
p.3551, l.17ff. state whether you refer to salinity units (psu) or whether you refer to normalized biases.
Answer:
We refer to non-normalized salinity biases. This sentence is no longer in the revised version for Biogeosciences and is included in the model/data comparison section in the other paper for Journal of Marine Systems.

12) Question
p. 3553, l.4/5 "simulated salinity is weaker than measured salinity" ?
Answer:
This sentence does not appear anymore in the revised paper.
"simulated salinity is weaker than measured salinity in the surface one, especially near the coast" has been changed to "simulated salinity is lower than measured salinity in the surface one, especially near the coast" in the paper to Journal of Marine Systems.
As explained in the submitted paper, this bias comes from the sea surface salinity corrections used in the model configuration.

13) Question:
p.3553, l18-20. It would be good to see whether the model has been spun up sufficiently, i.e. how large the remaining drift is in physical and biogeochemical model fields.
Answer:
The model has been spun up sufficiently. See our answer to question 1 and Fig. 1 above.

14) Question:
p.3554. It might be useful to quantitatively compare simulated and observed volumes with oxygen concentrations lower than, e.g., 50umol/l.
Answer:
We have another paper especially on O_2 concentrations and OMZ volume (Le Vu et al., in prep). The volume of simulated OMZ compared to CARS is underestimated as already explained in the submitted version of the paper, mainly on the continental shelf. If we remove O_2 concentrations for water depth lower than 200 m, this comparison is better.

15) Question:
p.3556, l.19-21. Could the overestimate of O_2 and underestimate of NO_3 be explained by too weak upwelling?
Answer:
We used the same configuration as in Veitch et al. (2009) in the Benguela upwelling system. In this simulation as well as ours, the horizontal resolution is 1/12°, so 8.5 km at 23°S. A finer resolution (1-2 km) would be better to capture the right intensity of the vertical velocity associated with the Ekman pumping. However in Veitch et al. (2009), they made a model/data comparison and found that the upwelling near the coats tends to be overestimated due to an underestimation of the wind drop-off. So the alongshore wind stress from QuickSCAT is too strong along the coast.

16) Question:
p.3556, l.23-28. This reads as if there is good agreement everywhere. The authors should say that the satellite data show highest chlorophyll over a wider area in the north, which is in contrast to the model behaviour that shows highest chlorophyll and a widest off-short extension in the south.
Answer:
In the revised manuscript, we improved the coupled simulation, and we also changed the initial and boundary conditions for both phytoplankton and zooplankton classes to improve the spatial distribution of chlorophyll concentrations. Indeed, southern boundary conditions impact over some hundred kilometers onto the domain due to intense alongshore current. These changes have clearly improved the chlorophyll concentrations and removed the different chlorophyll distribution between the northern and southern parts of the domain (Fig. 3 below). This figure is now part of the other paper (JMS) about the model/data comparison and sensitivity analysis.

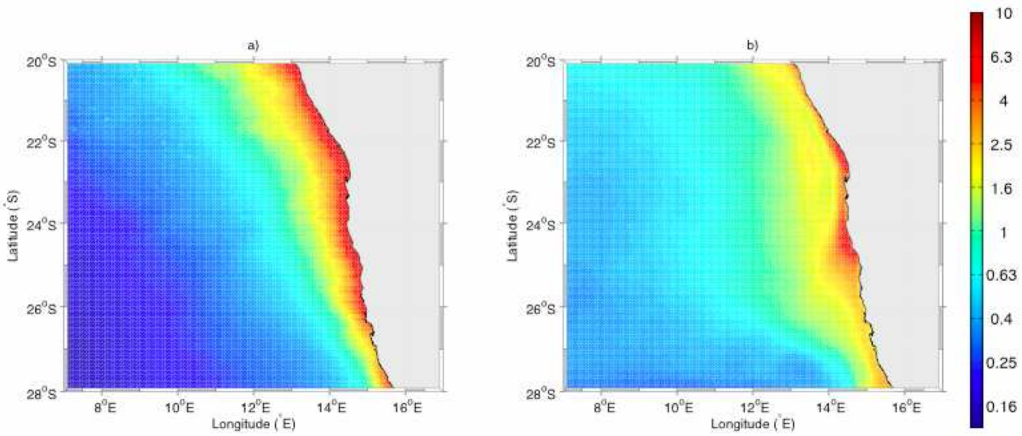

Figure 3. Annual mean of surface Chlorophyll-a concentrations (mg Chl m^{-3}) from SeaWiFS sensor (a) and the coupled model (b).

17) Question:
p. 3560, l. 29. I agree that the simulated values come closer to the in-situ values, but it should be said that there is still a factor 2 (or 100%) difference.
Answer:
We agree and have added your precision: ″there is still a factor 2 difference″ at the end of the sentence. This detailed comparison is now part of the other paper for Journal of Marine Systems.

18) Question:
p.3561, l.25-27. Why should the more complex parametrization of Freing et al. help? As I understand, Freing account for nitrification process only, and they apply their method to north Atlantic data only, i.e. they do not have to deal with very low oxygen.
Answer:
We agree and have removed this sentence in the revised manuscript.

19) Question:
p. 3563, l. 16 "a contribution decreasing towards the shore" l. 18 "represents a nitrate sink"
Answer:
Sorry, the explanation was not very clear in the submitted manuscript. To improve the understanding of the nitrate advection fluxes, we changed the representation from Fig. 18 of the submitted paper to Fig. 3 of the revised paper. We now represent the annual averages of zonal and vertical component of nitrate flux using vectors and meridional component of total nitrate flux divergence (in color). Between 100 and 600-m depth, the zonal current advects nitrate enriched waters toward the shore. Over the slope, one part is vertically advected and another part is poleward advected by the poleward undercurrent. Along the poleward undercurrent, poleward fluxes at the southern boundary (24°S) are higher than those at the northern boundary (22°S); that is why it generates a net sink of nitrate. Over the shelf, the zonal current is weaker than over the slope. Close to the coast, the vertical nitrate advection is principally supplied by the meridional component, due to the intense Luderitz cell South of Walvis Bay. We include these precisions in the revised version of our paper (lines 365-368).

20) Question:
p. 3563, l.25. Is the high f-ratio of about 0.9 in agreement with observations?
Answer:
Monteiro (2010) reported that the *f*-ratio between upwelling systems greatly varies from 0.1 to 0.8 and that mean values were between 0.2 and 0.3 in the Southern and Northern Benguela upwelling systems based on *in situ* data, respectively. Our studied area, Walvis Bay, is a particularly productive area within the Benguela domain. So, f-ratio in our studied area can greatly differ. For example, Rees et al. (2006) estimated a f-ratio up to 0.8 during AMT6 cruise (May 1998) in the Benguela upwelling system using nitrate and ammonium uptake with ^{15}N (figure 9a in their paper). So this value is far from the mean range between 0.2 and 0.3. The f-ratio can vary with time as well as in space due to NO_3 input variability in the euphotic zone associated with winter vertical mixing, eddies, filaments, etc. Estimations based on sparse *in situ* data should not capture this variability.
We validated the simulated primary production by comparing it with *in situ* measurements made off Namibia (see Section 2.4 of the revised paper), but we do not have enough data to really validate the simulated new production.

Rees, A., Malcolm E., Woodward I. J., Concentrations and uptake of nitrate and ammonium in the Atlantic ocean between 60°N and 50°S, Deep-Sea Research, II, 53, 1649-1665, 2006.

21) Question:
p. 3566, l. 27. Is there any formulation of anammox that does not require the simultaneous presence of NO_2 and NH_4?? Why is the Yakushev et al. formulation to blame here?
Answer:
This sentence has been removed. With our sensitivity analysis, we found a set of parameters for the denitrification and anammox processes which allows nitrogen fluxes closer to the observations. See Section 3.2 in the revised manuscript.

22) Question:
p. 3567, l.2. Woebken et al. reference is missing. (there may be others, but this is the only reference I checked - bad luck).
Answer:
Sorry, this reference has been added at the last moment by one of the co-authors. The references have been checked in the revised paper.

23) Question
p. 3567, l. 10 Please explain how exactly the "burial flux" is computed in the model without sediment?
Answer:
In the model, a cumulative layer at the sediment-water interface exists in which detritus or sinking particles are stored. Within this cumulative layer, PON just accumulates, without further interaction with the overlying waters.

24) Question
p. 3568, l.23. These numbers are given for 22-24S, while figure 18 shows numbers for the entire latitude range of the mode. This is confusing. Why not show and discuss either the total region or the Walvis Bay alone?
Answer:
We first estimated N offshore export at 10°E for the Walvis Bay area (22-24°S; Fig. 18 of the submitted paper) using the coupled model. And then, we extrapolated this estimation to the whole Benguela upwelling system in the text.
In the revised masnuscript, we replaced Fig. 18 with Fig. 3 and Fig. 4; we also changed the text in order to give a better explanation (see section 3.1).

25) Question:
p. 3569, l.15. Does this confuse mol N_2 versus mol N in Fig. 19? Otherwise I do not understand.
Answer:
In the submitted paper, all fluxes of Fig. 19 were in molN except for sea-air fluxes in mol N_2O. To avoid this confusion, all the fluxes are in molN, even sea-air fluxes in Fig. 8 of the revised manuscript.

26) Question:
p.3569. should "air-sea flux" read "sea-air flux"? It is somewhat difficult to understand that the model simulates N_2O concentrations too low by at least a factor 2 and at the same time simulated air-sea (sea air? this is unclear in the text) are too high, at least off-shore. What could be the reason for this apparent inconsistency?
Answer:

All fluxes at the ocean-atmosphere interface are expressed using the atmospheric convention, so a flux from the ocean to the atmosphere has a positive sign. In the revised manuscript, we checked all the term to avoid this confusion.

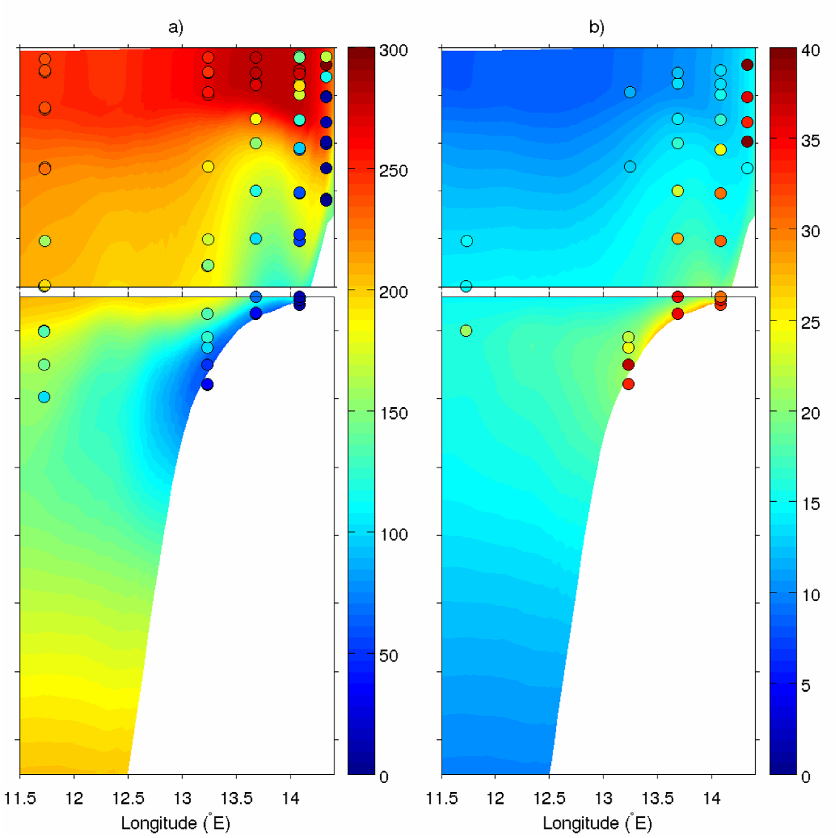

Figure 4. Oxygen (mmol O_2 m^{-3}), and nitrous oxide (10^{-3} mmol N_2O m^{-3}) concentrations estimated with the coupled model at 23°S, and averaged for climatological December. Colored circles for the FRS Africana (December 2009) data are overlaid on the modeled fields using the same color bar as the modeled fields.

When the model is compared with *in situ* data from FRS Africana cruise in December 2009, simulated monthly N_2O concentrations for December are clearly too low in the OMZ (Fig. 4 above), however these modeled concentrations are close to the data for the whole analyzed period and area (Fig. 5 below), especially in oxygenated water. This is the reason why N_2O fluxes to the atmosphere are similar to the observed ones. Unfortunately, we do not have data offshore to evaluate the performance of the model.

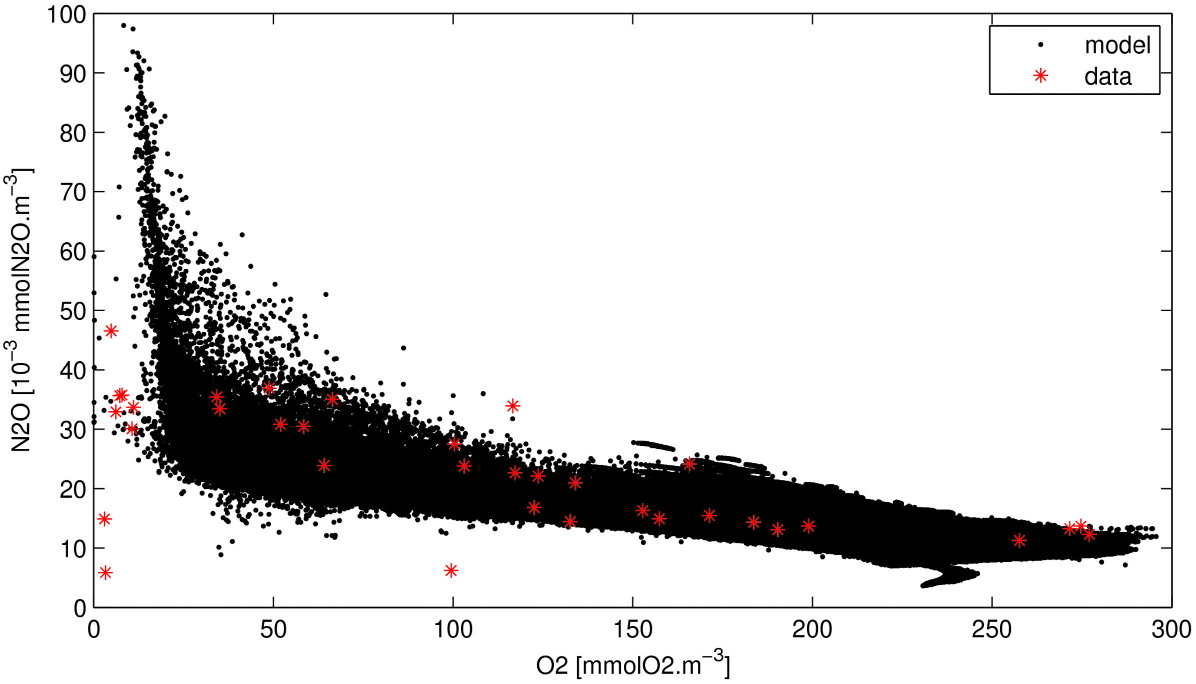

Figure 5. N_2O concentrations (10^{-3} mmol N_2O m^{-3}) as a function of O_2 concentrations (mmol O_2 m^{-3}) for simulated 3-days averaged fields (black; between Y12 and Y19) and in-situ data (red).

27) Question:

p. 3580, eq.A26/27. Is there no light inhibition of nitrification in the model? This might be useful to point out.

Answer:

There is no light inhibition of nitrification in the model and it is pointed out in the paper for Journal of Marine Systems.