Answers for Reviewer 2:

1) General Comments:

Gutknecht et al. present a complex and powerful model for assessing productivity and nutrient transformations in the Namibian upwelling system. The model is impressive in it's inclusion of a complex nitrogen cycle that includes rate limited denitrification and anammox reactions. To my knowledge, few, if any, 3D regional models incorporate these processes in such a detailed way. In general, the authors do a very nice job comparing the model with observational data. The model appears to simulate temperature, salinity, NO_3, and chlorophyll very well.
However, I do not think the authors spend nearly enough time explaining the substantial changes they have made to the original biogeochemical model by Koné et al. (2005).
Several new tracers have been added (O_2, DON, N_2O, NO_2) as well as multiple step denitrification, nitrification and anammox rate-limited reactions. The equations for these reactions appear to come directly from Yakushev et al. (2007) and are never explicitly discussed, justified, or validated. To this point, the authors do not state whether any testing of parameter values was performed despite the fact that Yakushev et al. present a 1D model for a very different aquatic ecosystem with many additional model components. Because the use of this complex nitrogen cycle will be of major interest to other 3D biogeochemical modelers, I strongly recommend moving the governing equations from the appendix to the main text in a separate section dealing specifically with denitrification, anammox, and how the tracers NO_2, NO_3, and NH_4 are calculated and sensitive to changes in the rate coefficients.
The need for additional N-cycle model evaluation becomes apparent towards the end of the paper when the denitrification and anammox rates calculated using the model are presented. The authors acknowledge that both rates are lower than observations, but do not point out that significantly more water column N is lost in their model through denitrification than through anammox, in opposition to the observational findings of Kuypers et al. (2005) in the Benguela upwelling. I very much recommend that they do some additional testing of the model sensitivity of the N-cycle rate coefficients. Also their N_2O production parameterization could be significantly improved, but probably not by using the parameterization of Freing et al (2009) as they suggest, but with one that explicitly includes N_2O production via denitrification. In addition, because anoxic sediments are an important feature of this region, including a sediment model that includes organic matter remineralization via aerobic processes as well as anoxic denitrification seems crucial for accomplishing the goals stated at the beginning of the paper: to investigate the full N budget in the Namibian sub-system of the Benguela Upwelling System.
Although this is the stated goal of the paper, this topic comprises a surprisingly small portion of the text due to a lengthy model validation section, which as mentioned previously, fails to validate or discuss some of the most interesting and complicated features of the model. I also have some concerns regarding the N budget. The authors never state whether the annual nitrogen fluxes through their region of interest balance. This would be a good indication that the model is in steady state. Using the numbers from Figures 18 & 19 for the top 100m over the slope, I calculate a net loss of inorganic N (-8.9×10^{10} mol N yr^{-1}) and net gain of organic N ($+2.38 \times 10^{10}$ mol N yr^{-1}). The magnitude of the net gain or loss is larger than some of the advective fluxes. I may have computed this incorrectly, but the authors should address this point and convince the reader they are presenting a balanced N budget. On a more conceptual level, I'm undecided about whether this paper, even after revision, can be successful in its aim to provide a complete and realistic N budget for the Benguela Upwelling system given the

lack of testing/validation of the denitrification and anammox modeling, it's lack of a sediment model, and weaknesses in the N_2O parameterization.
Their finding that EBUS can provide nutrients to ocean gyres is not terribly surprising and their number for this source will need to be revised as soon as a more complete model is available. In addition, although I think that evaluating the model is very important and requires some additional effort, it also makes the paper very long.
The application of the model to answer a scientific question does not come until very late in the paper when many readers interested in this particular topic will have already lost interest. I would recommend publishing a separate model validation paper and a shorter, to the point paper about the N-budget of the Benguela Upwelling System.

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 (JMS). 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. As you recommended, in this paper we moved the governing equations from the appendix to the main text. In this paper, we explain the substantial changes we have made to the original biogeochemical model by Koné et al. (2005). With the sensitivity analysis, we discuss the nitrification, denitrification and anammox rate-limited reactions from Yakushev et al. (2007), and we justify our parameter values in this paper for JMS.

- In the submitted paper, we used the same values as Yakushev et al. (2007) for denitrification and anammox processes. The denitrification and anammox rates calculated using the model were lower than the observations, with more N loss through denitrification than through anammox. For the revised manuscript for Biogeosciences and the JMS paper, we made a sensitivity analysis of the N-cycle rate coefficients and improved the denitrification and anammox rates. Estimations are now closer to the observations in Kuypers et al. (2005) and Lavik et al. (2008) for the Benguela upwelling. The simulated N loss through anammox is now of the same order of magnitude as through denitrification (see Section 3.2 of the revised paper).

- In the revised paper (lines 196-198), we improved the N_2O production parameterization, using the parameterization of Suntharalingham et al. (2000, 2012) that explicitly includes N_2O production via denitrification.

-We agree; anoxic sediments are an important feature of this region. We are working in including a sediment model, which considers organic matter remineralization via aerobic processes as well as anoxic denitrification. However, this represents a significant amount of work and will be the topic of future investigations and paper. To avoid confusion here, we removed the term "full N budget" and also the N-fluxes on the continent shelf where the sediment processes can have large impact.

-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.
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 submitted one was more confused as noticed by the reviewer. Now, the 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, it 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 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 estimations 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 OMZs. We show as well that these N losses off Namibia are not 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.

- The annual nitrogen budget through our studied region is balanced. See our answer to question 15 below.

-We agree; it is not surprising that EBUS provide nutrients to ocean gyres. However, we give an estimation of the total nitrogen (N) offshore export of at 10°E off Walvis Bay domain as well as the contribution of mesoscale activity (see above). To our knowledge, we do not know other estimation for the Benguela upwelling system.

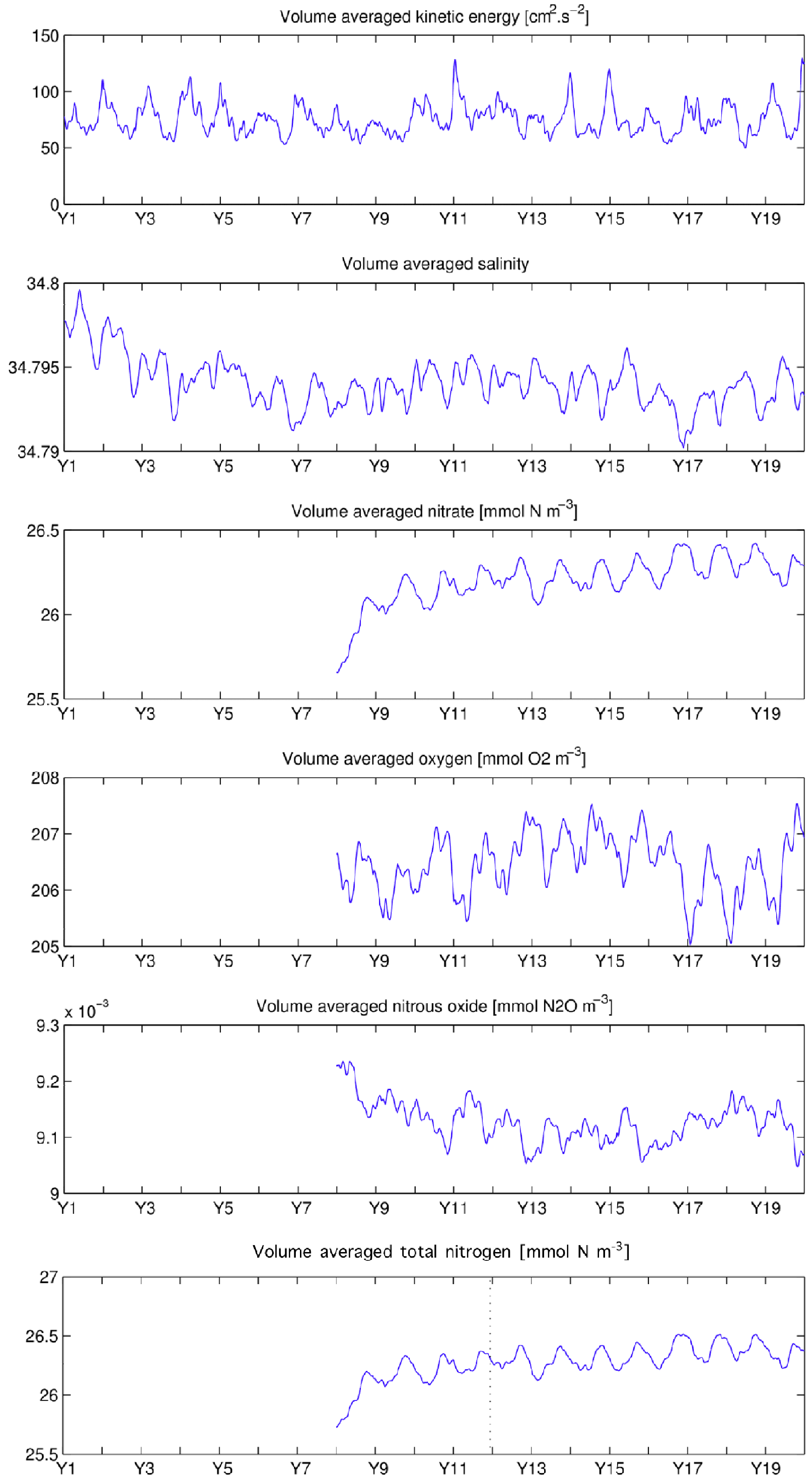

Figure 1. Time series of volume averaged kinetic energy, salinity, and nitrate, oxygen, nitrous oxide and total nitrogen concentrations for the 19-year Namibia simulation.

Specific Comments

1) Question:

p 3539 ln 9 : “alleviate” seems an odd word choice here, perhaps “potentially diminish” would be more appropriate. Starting the sentence with “However, losses of ...” would also put the sentence in context right away.

Answer:

“Losses of fixed inorganic N, through denitrification and anammox processes and through nitrous oxide (N_2O) emissions to the atmosphere, take place in oxygen depleted environments such as EBUS, and alleviate the role of these regions as a source of N” has been changed to “However, losses of fixed inorganic N, through denitrification and anammox processes, take place in oxygen depleted environments such as EBUS, and can potentially mitigate the role of these regions as a source of N to the open ocean.”, lines 26-28 of the revised paper.

P 3539 ln 18: awkward sentence - “over the first 100m over : : : over : : :”. I recommend changing the first use of “over ...” to “into the top 100m of the water column”.

Answer:

The abstract has been revised so, this sentence does not appear in the revised manuscript.

P 3540 ln 15: “of the global ocean, its estimated...” I recommend rewording to “of the global ocean, however we estimated it’s N_2O emissions using a parameterization based on oxygen consumption to be 4% of the ...”

Answer:

“The continental shelf off Walvis Bay area does not represent more than 1.2% of the world’s major eastern boundary regions and 0.006% of the global ocean, its estimated N_2O emission (2.9×10^8 molN_2O yr^{-1}), using a parameterization based on oxygen consumption, contributes to 4% of the emissions in the eastern boundary regions, and represents 0.2% of global ocean N2O emission.” has been changed to “The coastal domain off the Walvis Bay area considered in our study does not represent more than 1.2% of the global coastal upwelling areas; however a simple parameterization shows that its N_2O emissions (6.3 10^8 mol N yr^{-1}) could contribute to 4.4% of the global N_2O upwelling emissions.”, lines 50-53 of the revised paper.

p 3541 ln 20: “As for the other... equator: the Benguela ...” This sentence does not make sense. I believe it should read “As with the other EBUS, the trade winds maintain a horizontal pressure gradient along the coast associated with a coastal geostrophic current flowing towards the equator. In the BUS this coastal current is called the Benguela current and contains cold, nutrient-rich waters.”

Answer:

“As for the other EBUS, the trade winds maintain a horizontal pressure gradient along the coast associated to a coastal geostrophic current towards the equator: the Benguela current with cold and nutrient enriched waters.” has been changed to “As with other EBUS, the trade winds maintain a horizontal pressure gradient along the coast associated with a coastal geostrophic current flowing towards the equator. In the BUS, this coastal current is called the Benguela current and contains cold and nutrient-rich waters.”, lines 107-110 of the revised manuscript.

p 3541 ln 24: “under the form of eddies” should be “in the form of eddies”

Answer:

“under the form of eddies” has been changed to “in the form of eddies”, line 112 of the revised manuscript.

p 3542 ln 4 & 6: These sentences are a bit unclear in their description of where and when suboxic zones, anoxic zones, and anoxic events occur.

Answer:

We made the following change:

“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).”, lines 125-131 of the revised paper.

P 3542 ln 14: “alleviate” again this is an awkward word to use, “mitigate” or “diminish” would work better.

Answer:

“alleviate” has been changed to “mitigate”, line 133 of the revised manuscript.

<u>2) Question:</u>

p 3542 ln 19: “more efficient than CO_2...” this is too vague- more efficient than CO_2 at doing what?

Answer:

“N_2O is a greenhouse gas ~300 times more efficient than CO_2”has been changed to “N_2O is a greenhouse gas especially worrying as its global warming potential is ~300 times more efficient than carbon dioxide CO_2 (Jain et al., 2000; Ramaswamy et al., 2001).", lines 81-82 of the revised manuscript.

<u>3) Question:</u>

Pg 3543 ln 7: Questions two and three should be more specific – what is goal? Estimating the magnitude of the N loss and the N_2O production in this area? or the nature of the N loss? its seasonality? Etc...

p 3543 ln 15: Just stating that an N budget will be presented is again somewhat vague. Since this appears to be the main scientific goal of the paper a few more lines describing the approach, for instance, including which biogeochemical and physical mechanisms are included in a flux analysis performed to obtain the budget, would be helpful.

Answer:

In the revised manuscript, we changed this section. Three precise questions are now stated in the introduction. We do not present a full N-budget anymore (see the answer to reviewer’s general comment). We now focus our analysis on N offshore export, N losses via denitrification and anammox and N_2O fluxes at the ocean-atmosphere interface.

<u>Section 2</u>

<u>2) General Comment:</u>

I recommend describing the Namibian model configuration directly following section 2.1, which describes the hydrodynamical model. Then move onto the biogeochemical model is a larger change in topic.

Answer:

The Namibian model configuration is now detailed in the other paper for JMS. In this paper, we took your recommendation into account and moved the description of the biogeochemical model in the text (section 2).

3) General Comment:

It appears that some substantial changes were made to the biogeochemical model for this study. DON, NO_2, N_2O, and O_2 state variables were added as well as the rate limited nitrification, denitrification and anammox processes. I think greater explanation and the equations governing these processes and the new state variables should be presented in the main text. Many biogeochemical models parameterize nitrification and denitrification more simply (dependent on O_2, N, and detritus concentrations) and ignore anammox (PISCES, BEC, HAMOCC). Therefore it is a major achievement of this study that it uses a more complex and detailed representation of these processes.

However, the study cited as a description of the complex nitrogen cycle (Yakushev et al. 2007) is a 1D model designed to simulate a number of redox processes in the Black Sea. Simply citing this paper does not provide an adequate explanation of how this model works or these processes were incorporated into BioBUS. The Yakushev et al. model includes several other variables and processes which were not incorporated into BioBUS with rate coefficients obtained by tuning to model to produce observed concentration profiles. This is fine for that application but the authors must discuss what (if any) rates were changed from Yakushev et al., where those coefficients were obtained to begin with, and at a minimum a basic explanation of the reactions and equations. This is not trivial as multiple steps are involved due to the addition of the state variable N_2O. An entire section in the methods on the addition of new N-related tracers and the calculation of denitrification, anammox, and nitrification rates should be provided.

Answer:

All these explanations are now in the other paper for Journal of Marine Systems where we made an extensive description of the biogeochemical model, as well as a sensitivity analysis on key parameters, especially for denitrification and anammox processes (see also our answer to question 7).

4) Question:

P 3545 ln 8: It's interesting that a DON tracer what added. Often models use a slowly remineralizing, sinking, large detrital pool and a small more labile detrital pool that may or may not sink at all and is functionally a DON pool. Since we now have three pools of non-living organic N to consider within the context of a complex nitrogen cycle, a sentence or two comparing them in terms of remineralization, sinking rates, and their interactions would be helpful. Figure two shows large detritus being remineralized to NH4, Small detritus becoming DON, and DON being remineralized to NH_4. Is this correct? Why does large detritus become NH_4 directly but small detritus first becomes DON? A more detailed explanation and rationale for this complexity is warranted.

Answer:

Sorry, Figure 2 of the submitted manuscript was a bit confusing. In the revised manuscript, we put a new Figure 2 explaining the model in a better way. For example, Large and small detritus can produce labile DON and semi-labile DON. As labile DON is fast degraded in NH_4 (few days), we do not introduce a new state variable for labile DON.

5) Question:

P 3545 ln 16: A bit more explanation of the addition of O_2 as a state variable is necessary, at a minimum please provide a citation. [! I just saw that this is addressed in short appendix, not noted in this section of the paper. I recommend removing the appendix section and adding the relevant sentence here.] Also O_2 appears to be calculated in mmol O_2 m^{-3} (Table 1) which would be a much more intuitive unit to use when discussing O_2 concentrations in the text, as it's more easily comparable to the nitrogen units reported.

Answer:

The explanation for O_2 equation is now in the other paper for Journal of Marine Systems where we made an extensive description of the biogeochemical model. We added a citation for the O_2 equation (Peña et al., 2010) in the revised manuscript.

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.

<u>6) Question</u>

P 3545 ln 21: Figure one contains some confusing arrows that are not explained. Large phytoplankton become small detritus directly, but what process do the arrows branching off of phytoplankton as they flow toward zooplankton that are re-routed to detritus signify?

Answer:

In the revised manuscript, we put a new Figure 2 explaining the model in a better way as mentioned above. Black arrows represent the nitrogen-dependent processes, red arrows the oxygen-dependent processes, and blue arrows the processes linked with N_2O production. To simplify the representation of all interactions between variables, arrows from or to a grey rectangle act on all variables included in this grey rectangle. For example, the arrow between nutrients and phytoplankton (assimilation) is a simplification of 6 interactions: NO_3^- to P_S, NO_3^- to P_L, NO_2^- to P_S, NO_2^- to P_L, NH_4^+ to P_S, NH_4^+ to P_L.

<u>7) Question:</u>

P 3546 ln 16: In the parameter adjustment experiments, was each parameter changed independently? And why were certain parameters chosen for sensitivity analysis and not others? There are so many interrelated processes occurring in this model it seams that changing some together or those that are the least well known or constrained by observations would make sense. For instance, when you compare changes in the DON mineralization parameter KND4 to the distribution of NO_3 and O_2, wouldn't it make sense to also consider some of the other parameters affecting DON on it's path to becoming NO_3 (DON ! NH4, NH4 ! NO2, NO2 ! NO3). Testing values for either of the nitrification rate parameters could be just as useful KND4 as they are relatively uncertain and affect both NO_3 and O_2 directly. Just a sentence or two explaining why some parameters were tested and others not (if they were not) would be appreciated.

Answer:

For the revised manuscript for Biogeosciences as well as for the Journal of Marine Systems paper, we performed a sensitivity analysis and included more parameters, especially those for denitrification and anammox processes as we have some estimations (Kuypers et al., 2005; Lavik et al., 2008; and their personal communication on the measurements they made). These parameters have been tested independently, and then in combined set (see Table 1 below). The last parameter set (last line) in this Table is the one we used for the short data/model comparison (section 2.4) and the N-fluxes modelled estimations (section 3) in the revised manuscript.

This sensitivity analysis is described and explained in the paper for Journal of Marine Systems.

	N loss (10^{11} mmolN/yr)		N loss (10^{-3} mmolN/m^3/d)		Max N loss (10^{-3} mmolN/m^3/d)	
	denitrification	anammox	denitrification	anammox	denitrification	anammox
Reference	0.1941	0.0094	2.3373	0.1189	56.8502	16.1623
Nitrif/10	0.4486	0.0944	7.1576	1.522	80.2318	39.6745
Anam/10	0.1942	0.001	2.3755	0.0124	57.6255	1.7802
Anam*10	0.1826	0.0774	2.3446	1.0185	51.669	89.072
Denitr*10	1.9041	0.0621	24.8874	0.9284	574.8622	56.7271
Denitr/10	0.0055	0.0005	0.0693	0.0056	3.6874	0.6753
RemO2*10	0.001	0.0002	0.1524	0.0063	17.885	2.4752
Wsed*2	0.1039	0.0038	2.0272	0.0743	69.6408	7.7673
Nitrif/10 + Anam*10 + Denitr*10 + Wsed*2 + RemO2*2	3.1947	4.4667	18.3242	22.3265	139.9319	202.7676
Anam*10 + Denitr*10 + Wsed*2 + RemO2*2	1.9931	0.4813	9.0558	2.2756	97.7047	129.5482

Table 1: N loss due to denitrification and anammox processes for different values of Nitrif (nitrification rate), Anam (anammox rate), D (denitrification rate), Rem (remineralisation rate), Wsed (sedimentation velocity for large detritus).

8) Question:

P 3553 ln 12: Once I searched and finally found the paper referred to (Monteiro and van der Plas 2006) I found the comparison of the mooring data and figure 11 quite good. But this in situ data should be presented and printed in THIS paper. The reader should not have to search and obtain a relatively obscure article and flip to a figure in the middle of the article to see if the what the authors say about how their data compare to observations is reasonable. If it's not possible to publish a figure with the mooring data in this article, the paragraph should be reworded to simply describe the temporal dynamics in the model data and state that this is in good agreement with mooring data published in (Monteiro and van der Plas 2006). Also, why is there a jump in figure number from 7 to 11. Figure 11 should become figure 8.

Answer:

We removed Figure 11 (the same figure as in Monteiro and van der Plas, 2006) for the revised manuscript. We contacted Pedro Monteiro to have access to the data or to have their agreement to include their figure in our paper for Journal of Marine Systems.

9) Question:

P 3554 ln 27: Could slightly too high oxygen and too low NO_3 between 200-40m (Fig. 8) be due to underestimated nitrification? Your parameterization doesn't allow it to proceed in the euphotic zone, but there have been several studies that have observed nitrification in low to even moderate light. However, it doesn't sound like NH4 concentrations are high enough here to increase nitrification rates.

Answer:

Slightly too high O_2 and too low NO_3 between 200-400m (Fig. 8 of the submitted paper) are not due to underestimated nitrification. Fig. 8 compares simulated O_2 and NO_3 with *in situ* data (METEOR 57/2 data, in February 2003). It would be better to compare model with climatology, as made in Fig. 9 of the submitted manuscript. Please find below (Fig. 2), the new comparison between CARS database (2009) and simulated fields using the improved simulation. This new figure is now in the Journal of Marine Systems paper (model/data comparison section). As you can see, the model gives satisfying results, except for O_2 on bottom continental shelf waters. We made a sensitivity analysis on the different parameters which have an impact on O_2 concentrations. We concluded that sediment processes are necessary to improve O_2 over the continental shelf ($z < 200$ m). We are working at the moment to include a sediment module; it is a very consequent work and thus will be presented in a future article.

Concerning nitrification, as can be seen in Sect. 3.2 and Table 3 of the revised paper, simulated aerobic NH_4^+ and NO_2^- oxidation rates have similar rates as reported in Namibia and other EBUS.

Yes, our nitrification parameterization allows it to proceed in the euphotic zone, but low NH_4 concentrations limit nitrification in the surface; simulated nitrification rates present a subsurface maximum located between 20 and 80-m depth.

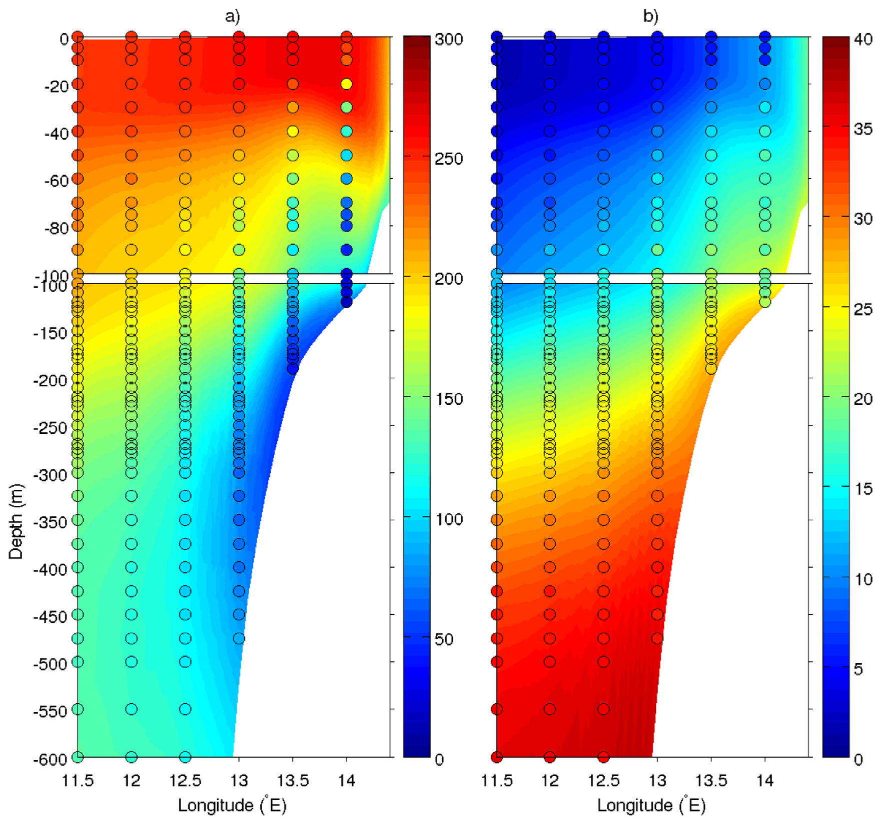

Figure 2. Simulated annual mean of (a) oxygen (mmolO_2 m^{-3}) and (b) nitrate (mmolN m^{-3}) concentrations at 23°S and between 0 and 600 m. Colored circles for the annual mean of CARS database (2009) are overlaid on the simulated fields using the same color bar as the simulated fields.

<u>10) Question:</u>
P 3557 ln 24: Although your model produces a deep chlorophyll maximum, the gradient between the surface and deep chl max is not nearly as steep as in the observations. This is fine but should be noted in this assessment section.
Answer:
We agree; we added your comment to the paper for Journal of Marine Systems.

<u>11) Question:</u>
P 3558 ln 19: "Spatial variations are important." An additional figure would be really helpful for this paragraph comparing model and the observations described. Also, why not compare with a satellite algorithm for primary production?
Answer:
As the *in situ* data from Barlow et al. (2009) are sparse in space and time (two different years: winter 1999, summer 2002), we did not make a figure. We preferred to compare the different ranges between the model (climatological configuration) and the observations in Table 1 of the revised paper.
We did not compare with satellite algorithm for primary production from Silio-Calzada et al. (2008) in the Benguela upwelling system (BUS) because their algorithm has not been validated with *in situ* data in the BUS. Tistone et al. (2009) pointed out also the lack of *in situ* data in the Benguela upwelling system to validate the satellite algorithms for primary production in this area. So, we preferred to use *in situ* data for primary production.

12) Question :

P 3561 ln 12: Over what time scale is the modeled data presented? Is this an average for climatological December, or the year, or a shorter time-scale? Temperature agrees well in Figure 17 a, but none of the other variables agree well.

Answer:

Figure 17 in the submitted manuscript presents simulated fields, averaged for climatological December month. We added this precision in the model/data section of our paper for Journal of Marine System.

Please find below (Fig. 3) the new comparison between model and data using the parameterization from Suntharalingham et al. (2000, 2012). We acknowledge that modelled and *in situ* data do not agree well because we compare climatological fields with *in situ* measurements for a specific year (2009). However, here we wanted to show that simulated N_2O concentrations have similar values as compared to *in situ* N_2O data when simulated O_2 concentrations are close to measured N_2O concentrations. It is the case for the oxygenated water column and for the waters close to the sediment onto the continental slope, with simulated values up to 30 10^{-3} mmol N_2O m^{-3} and *in situ* values up to 40 10^{-3} mmol N_2O m^{-3}.

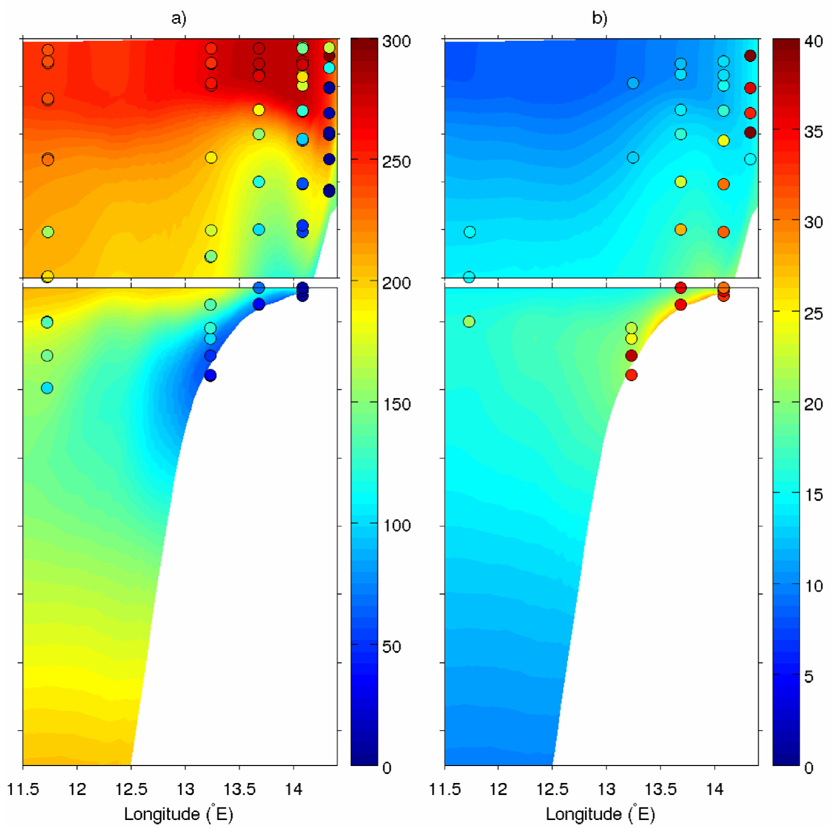

Figure 3. Oxygen (mmol O_2 m^{-3}), and nitrous oxide (10^{-3} mmol N_2O m^{-3}) concentrations estimated with the coupled model at 23°S, and averaged for climatological December. Colored circles for the FRS Africana (December 2009) data are overlaid on the modeled fields using the same color bar as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

13) Question:

P 3561 ln 21: "Simulated N_2O concentrations have similar values as compared to data for waters with O_2 > 2.6 ml L^{-1}". To me it looks like N_2O agrees well only at O_2 levels above _5 ml L^{-1} and that ignores the high N_2O throughout the surface waters close to the coast.

Answer:

It is not obvious to compare climatological fields with *in situ* measurements sampled under particular conditions (Fig. 3 above) because the coupled model is forced using climatological forcing. So, comparison can become clearer using other representation types. Fig. 4 shows that simulated N_2O concentrations are close to *in situ* data when O_2 concentrations are higher than ~ 120 mmol O_2 m^{-3} (or ~2.6 ml O_2 l^{-1}), and simulated N_2O concentrations are underestimated for O_2 concentrations below this limit. However, N_2O values up to 30 10^{-3} mmol N_2O m^{-3} are simulated on the bottom waters of the shelf for a climatological December, close to *in situ* values of 40 10^{-3} mmol N_2O m^{-3}.

Regarding the field for the full analysed period (Fig. 5 below) without monthly average, simulated N_2O concentrations follow the same trend as *in situ* measurements as function of O_2 concentrations. Simulated values reach 90-100 10^{-3} mmol N_2O m^{-3}. However, we do not have enough *in situ* measurements to validate our fields at low oxygen concentrations.

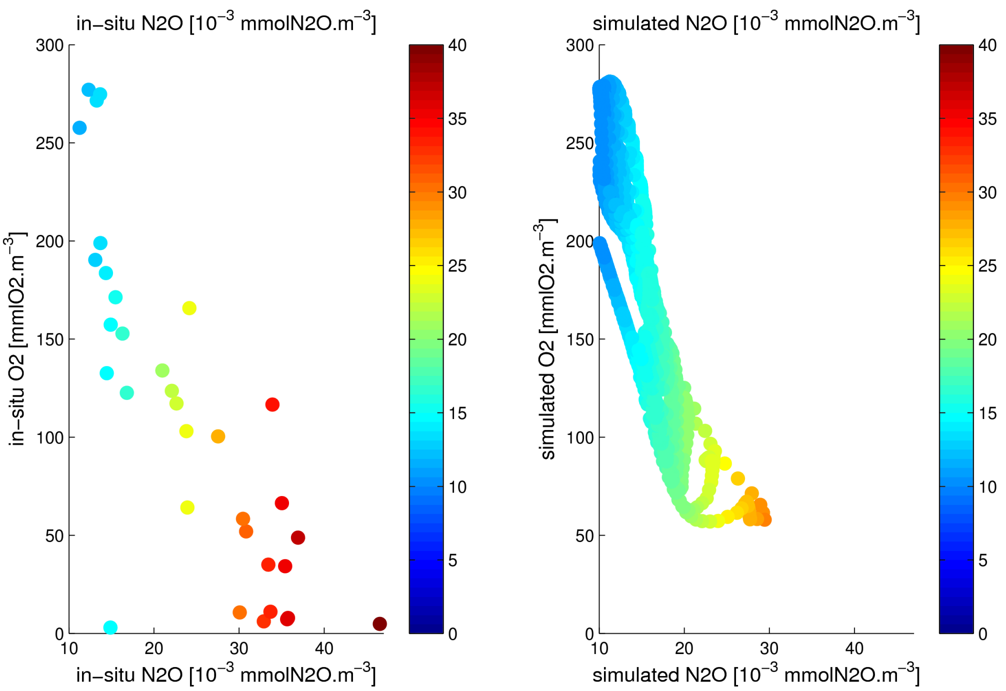

Figure 4. Left: In situ N_2O concentrations (10^{-3} mmol N_2O m^{-3}; abscises axis) as a function of in situ O_2 concentrations (mmol O_2 m^{-3}); from FRS Africana cruise in December 2009. Right: Simulated N_2O concentrations (10^{-3} mmol N_2O m^{-3}; in color) as a function of simulated N_2O and O_2 concentrations; for a climatological December (Y12-Y19). In color are also represented in situ (left) and simulated (right) N_2O concentrations.

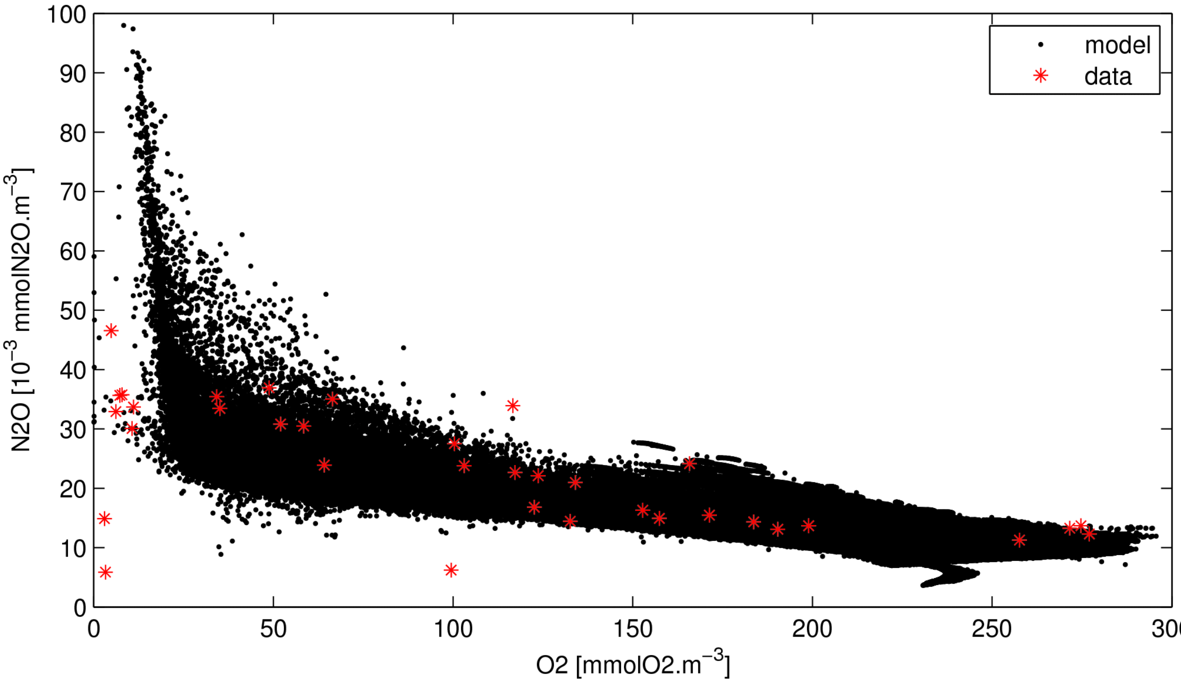

Figure 5. N_2O concentrations (10^{-3} mmol N_2O m^{-3}) as a function of O_2 concentrations (mmol O_2 m^{-3}) for simulated fields (black; between Y12 and Y19) and in situ data (red).

14) Question:

P 3561 ln 27: I don't know if using the Freing N_2O parameterization will improve the modeled N_2O profile very much. It also only takes nitrification into account. A simple parameterization based on Suntharalingham et al. (2000) that accounts for N2O production via both nitrification and denitrification can be found in Dutreuil et al. 2009 (Biogeosciences, www.biogeosciences.net/6/901/2009). Another problem could be that in the current parameterization no nitrification can take place in the euphotic zones, though this is sometimes observed (Dore and Karl 1996, Wankel et al. 2007). Maybe altering the light/depth dependence on nitrification rates would help reproduce observed distributions.

Answer:

We removed this sentence in the revised manuscript. In this revised version, 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.

The light/depth dependence on nitrification is not directly taken into account in our biogeochemical model.

15) Question:

P 3563 ln 17: "poleward undercurrent..." Why not specify if the meridional advection has a net flow to the north or south? From the description is sounds like the alongshore Benguela current is a net flow from the south into the budget box, and the 100-600m box over the slope is a net southward flow. Also what is meant by "sink for the studied area with a maximum value"? Do these fluxes balance to a net zero over a year? After a calculating the sum of the organic and inorganic fluxes into and out of the top 100m of the slope box it appears there is an imbalance (-8.8 x 10^{10} mol N yr^{-1} inorganic, + 2.38 x 10^{10} mol N yr^{-1} organic). Whether the fluxes balance over an annual cycle for each box should be addressed.

Answer:

Sorry, this part was not very clear. Fig. 18 of the submitted paper represented annual nitrogen budget performed around Walvis Bay (between 22°S and 24°S) for physical fluxes for nitrates, DOM, and POM. For zonal and vertical fluxes, we reported the fluxes (arrows) but, for the meridional term (cross-circles), we just reported the divergence, so positive is a net nitrogen source and negative is a net nitrogen sink. However this figure was very confusing and overloaded in number. To improve the understanding of the nitrate advective 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. On the poleward undercurrent, poleward fluxes at the southern boundary (24°S) are higher to those at the northern boundary (22°S) which 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 supplied by the meridional component principally, due to the intense Luderitz cell South of Walvis Bay.

The balance of the nitrogen budget has been checked on-line. For each time step, the error is about 10^{-16} mmol N m^{-3} s^{-1} for nitrogen. Considering the time step of the model of 900 seconds, so 34560 time steps in one climatological year (360 days), the accumulated error is equal to ~ 3 10^{-9} mmol N m^{-3} yr^{-1} to balance the nitrogen budget. This error is satisfying as compared to the smallest fluxes of the coupled model (ex: anammox: ~10^0 mmol N m^{-3} yr^{-1}). We can not give a nitrogen budget over a year as we did not save all fluxes due to lack of computing time and memory space. In order to avoid any confusion, we removed the term "full nitrogen budget" in the revised manuscript.

16) Question:

P 3564 ln 1. The areas used in calculation of PP in these comparisons are not well explained. The area of the Walvis Bay used in the budget (I think this is the area used for the first two numbers presented) seems to be smaller and more productive than the "entire Walvis Bay" referenced a bit later (ln 9), but how does this compare to the area of the BUS used to calculate PP by Ware, Carr, Tilstone and Brown? Even some rough estimate of the approximate differences in areas would be helpful here. Or maybe a figure.

Answer:

This comparison is now part of the model/data paper for Journal of Marine Systems. However, we agree that the comparison was not very clear. So, we changed the presentation of the results and the text has been clarified.

In the revised manuscript (section 2.4), the modelled PP is compared to *in situ* data from Barlow et al. (2009) for the same area, and it has been added in Table 1 for the summer (February-March) and winter (June-July) seasons.

17) Question:

P 3566 ln 12: The reference to Kuypers et al. (2005) is very confusing. It appears the authors are providing 0.075 – 0.25 mmol N_2 m^{-3} d^{-1} as the in situ rates of N_2 formation associated with denitrification. However, I cannot find any denitrification rates in Kuypers. In fact, Kuypers et al. finds little to no evidence for significant denitrification, attributing the majority of fixed nitrogen loss to anammox. Lavik et al. (2008) does present but not discuss one denitrification measurement but the provenance of the cited rates is not clear. Also, it should be noted that the BioBUS model predicts significantly higher denitrification rates than anammox rates for fixed removal from the water column in contrast to Kuypers et al. This seems to warrant some testing of parameter values used in the complex N cycle.

Answer:

We used data from Kuypers et al. (2005) and Lavik et al. (2008), and in addition, we had also access to estimations made by G. Lavik and M. Kuypers during these two cruises (RV Meteor, AHAB1). This information has been provided by G. Lavik who is a co-author of our paper. In the revised paper, we had this comment: “G. Lavik and M. Kuypers, pers. comm.”(line 488).

In the submitted paper, simulated denitrification and anammox rates were lower than observations, with significantly more N loss through denitrification than through anammox. In the revised manuscript, we made a sensitivity analysis of different parameters of our biogeochemical model and improved the modelled denitrification and anammox rates. Estimations are now closer to the rates from Kuypers et al. (2005) and Lavik et al. (2008) in the Benguela upwelling system. The N loss through anammox is now of the same order of magnitude as through denitrification (see Section 3.2 of the revised manuscript).

18) Question:

P 3567 ln 10: Some conclusion should be given as to whether the PON/POC reaching the sediments is reasonable compared to observations. Lots of numbers with different units are given and it gets confusion. It appears that the BioBUS model overestimates PON/POC burial on the continental shelf, but some clarification should be provided about which areas should be compared directly between the model and observations.

Answer:

In the submitted manuscript, we compared export production at 100-m depth with the estimation made in Monteiro (2010) using a model box. Our estimation has the same order of magnitude as those from Monteiro (2010), with an overestimation by a factor ~ 4. However, the area around Walvis Bay is very productive (see our comment on primary production), so export production is high compared to the whole northern BUS by Monteiro (2010). This overestimation of export production at 100-m depth is specified in the revised paper (lines 445-448).

19) Question:

P 3568 ln 25: Why assume a horizontal surface for the S. Atlantic gyre equivalent to that of the N. Atlantic? Is the same area being used or just the same approach for estimating the area? Why not just specify the area you estimate and use for the calculation.

Answer:

We used the same approach as Charria used for the N. Atlantic.

To avoid confusion, we changed our approach and changed “Assuming a horizontal surface for the South Atlantic Subtropical Gyre (7.7 10^6 km^2) equivalent as the one considered in Charria et al. (2008b) for the North Atlantic Ocean,” by “Considering a horizontal surface for the South Atlantic Subtropical Gyre of 9 10^6 km^2 (based on the South Atlantic Subtropical Gyral Province from Longhurst, 1998), …”, lines 406-407 of the revised manuscript.

20) Question:

P 3570 ln 27: Why compare your modeled N_2O fluxes with observations if the conditions are not similar (oligotrophic)?

Answer:

Obviously, N_2O data measured over the South Atlantic Subtropical Gyre during AMT 12 and AMT 13 cruises are not directly comparable with data estimated off our studied area because we are not under oligotrophic conditions. However, we wanted to have an idea of the values measured in the open ocean, if the open ocean represents a N_2O source for the atmosphere or a sink. Using AMT 12 and 13 cruises, we show that even in the open ocean, sea-air fluxes are weakly positive, so the open ocean is a weak N_2O source (flux from the ocean to the atmosphere).

<u>21) Question:</u>
P3570 ln 15: An important deviation? Section 4.5 does not clarify what is meant here. It refers to figure 17 where a large difference between modeled N_2O and measured N_2O is shown, but I don't see why surface fluxes cannot be calculated as they would be at the other stations.
Answer:
Close to the coast, some *in-situ* N_2O concentrations (averaged over 3 measurements) have an important standard deviation (> 25% of the mean value); the estimation made close to the surface has even a higher standard deviation than the mean value. It is the reason why we do not use these measurements to estimate sea-air fluxes.

<u>22) Question:</u>
Pg 3571 ln 15: The model values seem on the low side of the other estimates mentioned. Especially the ones from the Mauritanian upwelling.
Answer:
Our coupled model estimates averaged ocean-atmosphere N_2O fluxes up to +8 10^{-2} mmol N_2O m^{-2} d^{-1}, close to the coast, which is comparable to measurements made in other EBUS (see Table 4 of the revised manuscript).

<u>23) Question:</u>
P 3571 ln 21: Why not area-normalize these modeled fluxes as well as the fluxes by Nevison et al.? This would reduce the amount of information presented, which is confusing, and be much simpler and to the point.
Answer:
To reduce the amount of information in the text and easily switch between integrated and area-normalized estimations, we presented the results in Table 5 in the revised manuscript.

<u>24) Question:</u>
P 3572 ln 11: I'm puzzled because the model results show that the N_2O fluxes from Walvis Bay are quite high compared to other upwelling areas. But N_2O formation associated with denitrification is not included in the N_2O parameterization and modelled N_2O fields are much lower than observations (50%) in low O_2 regions (Fig 17). Are these high fluxes reasonable given that N_2O seems to be underestimated in the model?
Answer:
Simulated N_2O concentrations are too low in the OMZ for the climatological December month for the model when comparing with *in situ* data; however the model is able to simulate high N_2O concentrations (see Fig. 5 above). Moreover, simulated N_2O concentrations are close to the data in the oxygenated waters and so close to data in the surface waters. It is the reason why N_2O fluxes to the atmosphere are similar to the observed ones.

<u>25) Question:</u>
P 3574 ln 28: Again, Freing et al (2009) does not include denitrification in its calculation of N2O, which I think is what is implied by "...N_2O formation process associated with suboxic processes" in the previous sentence. I recommend looking at Dutreil et al. (2009).
Answer:
We agree and removed this sentence in the revised version of our paper as mentioned above.

<u>26) Question:</u>

p 3575 ln 1: Making the N cycle even more complex in your model may not be the most effective way to increase anammox rates. What about testing changes in the rate coefficients governing NO_2 production (1st stage of nitrification, etc)?

Answer:

We agree and changed this sentence in the revised manuscript. We carried out a sensitivity analysis also on the anammox process parameters. The anammox rates in the model are now closer to the observations of Kuypers et al. (2005) and Lavik et al. (2008) made in the Benguela upwelling system.