Report #1

Anonymous Referee #1

General Comments

This work describes the development and calibration efforts of a new biogeochemical model, focused on the O_2 and N cycle in the OMZ of the Benguela Upwelling System.
The development and testing of this biogeochemical model is an interesting and comprehensive work. The synthesis of processes and various parameterizations is well discussed and integrated in the model and sensitivity tests. An additional value of this work is the comparison with a broad set of in situ data, in particular related with the N cycle, although they are still sparse and rare.

The authors nicely show that physical terms (advection and mixing) are driving the oxygen content in the OMZ, and that biogeochemical processes are maintaining its level (Fig. 14). But since the physical conditions are so important, what about testing the boundary conditions, in particular the O_2 concentrations introduced to your regional domain, and the physical parameters (mixing terms). In section 5.3, you mention that a more developed OMZ over the shelf would significantly improve your estimates of N_2O production and outgassing. This could be easily tested with a sensitivity test about your boundary conditions, or even a test case of restoring the oxygen concentration only, and see whether the fluxes mentioned by Suntharalingam et al. are achievable with your model. A discussion about the relative importance between all your efforts about the biogeochemistry parameterization and the physical model parameterization (mixing coefficients) would be necessary. What is the sensitivity of all your calibrations relative to the physical settings?
Overall, the development and calibration work of such a complex biogeochemical model is a lot of work that will be of interest for the modeling community. Nevertheless, I would encourage the authors to develop the concluding part about how to improve the future work and model development in regard to their own model caveats and advantages.
I would recommend publication of this work after a strong effort of revising these few comments and discussion points.

Answer:

The poleward undercurrent coming from Angola supplies the Namibian system in oxygen-depleted waters. Monteiro and van der Plas (2006) showed that the oxygen fluxes at boundary conditions (especially the northern boundary) govern the Namibian OMZ formation, while local biogeochemical oxygen demand acts on its persistence and intensity. Our modeling study confirms that poleward meridional advection is the main sink term of oxygen maintaining the OMZ offshore of 300 m isobath, while vertical mixing acts to dissipate the OMZ. Inshore of 300 m isobath, biogeochemical activity is the main process depleting oxygen in our model.

We believe that a test case of restoring the oxygen concentration is problematic for our study as it will be difficult to have a closed O_2 budget over a year. Indeed, O_2 will be removed artificially through this restoring term. We have instead performed 3 additional simulations with different O_2

initial and open boundary conditions. We decreased the O_2 concentrations with a 25%, 50% and 75% factor for O_2 concentrations below 60 mmolO_2/m^3 (the upper level for the hypoxia) as compared to the CARS 2006 climatology. This climatology was used as initial and open boundary conditions for the Reference simulation. We chose the extreme factor (75%) by comparing this climatology with sampling conducted during the AHAB1 cruise (January 2004) under particularly oxygen-depleted conditions.

A decrease of O_2 concentrations at the northern boundary in the Namibian configuration (at 19°S, Fig. 1 below) only has a small impact on the shelf break and no impact on the shelf at 23°S (Fig. 2 below). So meridional advection of oxygen-depleted waters is a necessary condition to form the Namibian OMZ but does not drive the intensity of the OMZ off Namibia (see Fig. 2 below), confirming the results of Monteiro and van der Plas (2006). In the submitted paper we show that the biogeochemical activity was the dominant process to maintain minimum oxygen concentrations on the shelf (inshore of the 300m isobath). This result is strengthened by the supplementary sensitivity tests performed here. Input fluxes at boundary conditions do not represent a good candidate to better simulate the OMZ over the shelf or to improve our estimates of N_2O production and outgassing. We added this information in the revised version of our paper: "Supplementary sensitivity tests (not shown) using different O_2 concentrations at the northern boundary (here 19°S) were also performed. These tests confirmed that meridional advection of oxygen-depleted waters at this northern boundary represents a necessary condition to form the Namibian OMZ but does not drive the intensity of the OMZ off Namibia, confirming the results of Monteiro and van der Plas (2006)." (Lines 905-910).

From the sensitivity analyses in the submitted manuscript as well as the supplementary sensitivity tests on O_2 concentrations at the northern boundary, we concluded that the biogeochemical parameterizations are of major importance in EBUS and associated OMZs. That is why in future work we head towards sediment processes to improve the OMZ representation over the shelf.

In this study, we based our work on the ROMS-AGRIF nested configuration of South Africa region (SAfE for South African Experiment) developed and evaluated by Penven et al. (2006b) and Veitch et al. (2009) for the physics. We therefore used the SAfE outputs to provide the initial and open boundary conditions (physical state variables) of the Namibian configuration. We used the same atmospheric forcing and parameters for the physics (mixing terms …) to be consistent with the study of Veitch et al. (2009). In our study, we then decided to put our effort into biogeochemistry and analyse the impact of the values of the biogeochemical parameters.

In the revised version of the manuscript, we added a paragraph on the importance of the physical model parameterization (mixing coefficients) in the discussion section, although this was not the original focus of our inquiry. We used an already-validated configuration for the physics in the studied area and the associated physical settings (Veitch et al., 2009). We do however acknowledge that the physical model parameterization may have some influence on the results as suggested by e.g. Duteil and Oschlies (2011). (see lines 911-918).

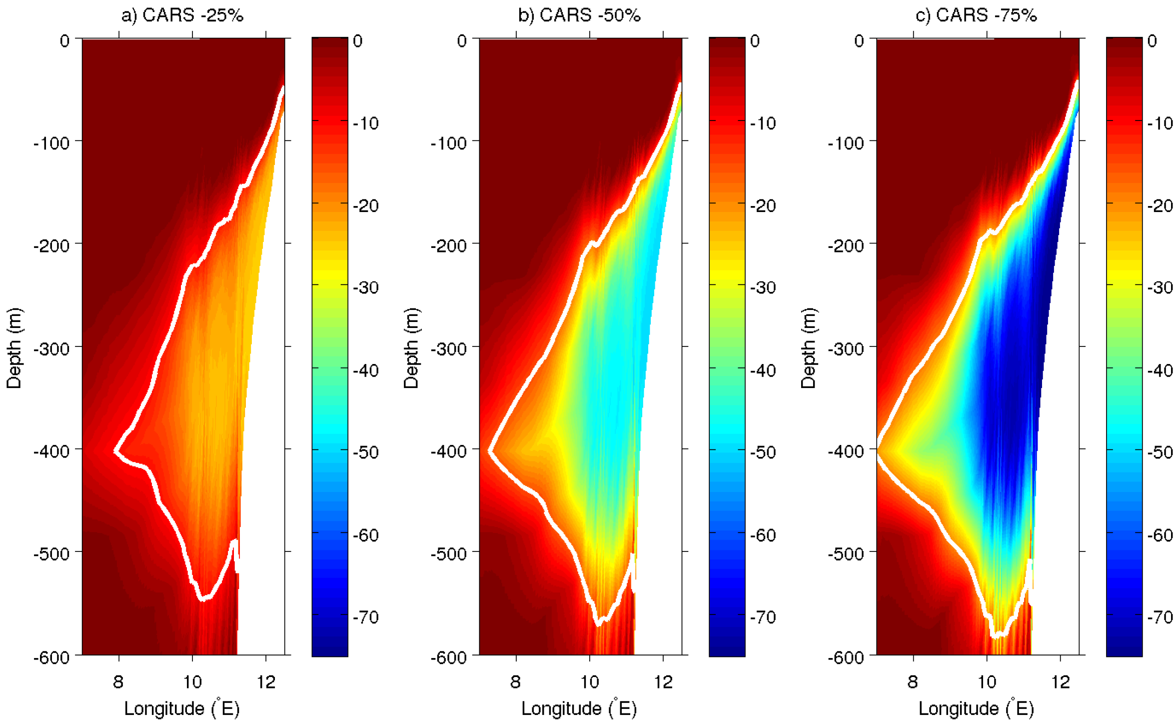

Figure 1 : Spatial oxygen distribution at the northern boundary conditions (19°S) using a decrease (in %) of oxygen concentrations with a 25% (a), 50% (b), and 75% (c) factors for concentrations below 60 mmol O_2 m^{-3} (upper limit for hypoxia in white isocontour) as compared to the CARS 2006 climatology.

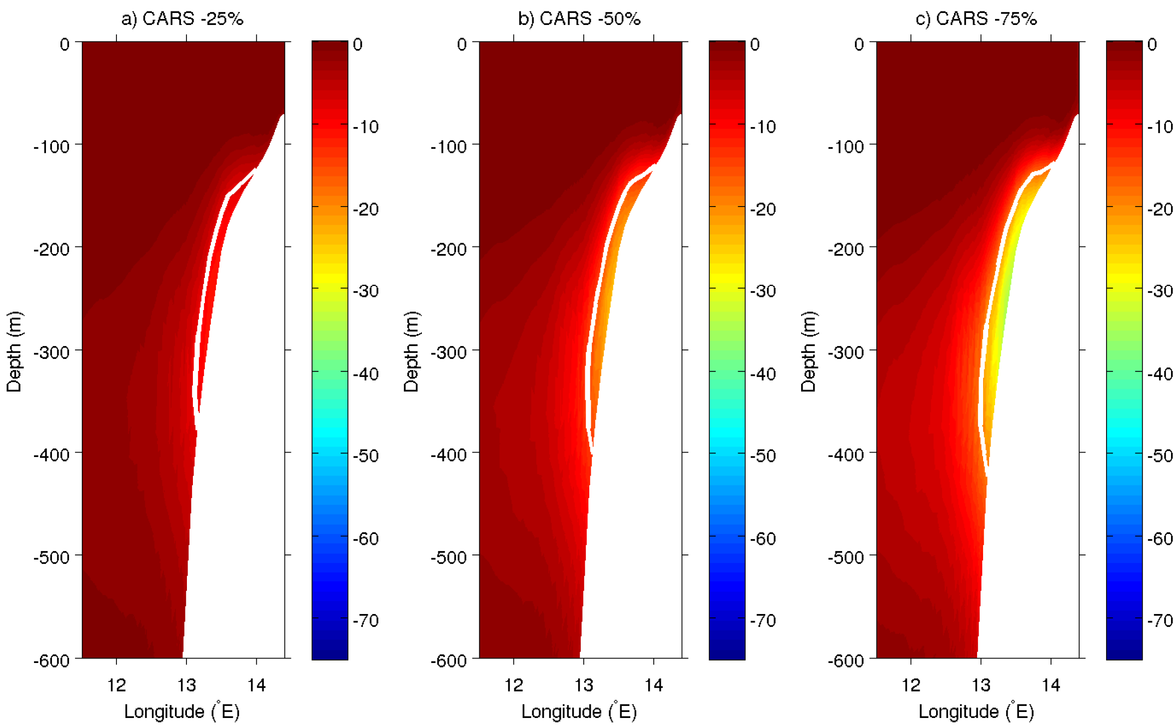

Figure 2 : Impact (decrease in %) on spatial oxygen distribution at 23°S as compared to the Reference simulation We applied an O_2 concentration decrease with a 25% (a), 50% (b), and 75% (c) factors in initial and open boundary conditions. The white isocontour stands for the upper limit of hypoxia (60 mmolO_2 m^{-3}).

Specific comments

Section 3.5 and Figure 9: The fig 9a and b do not seem to me appropriate to really compare model and data on the point to point basis. It would be worth commenting why the model produces much higher N_2O concentrations (> 50 $10e^{-3}$ mmol /m^3) at low O_2 concentrations (but still > 10 mmol O_2 /m^3) than observed (the rising trend along the y-axis). Is this realistic in other oceanic regions? In situ data are rare, and it is therefore difficult to evaluate there significance. But can the model achieve reproducing data points like the two ones in lower left corner of Fig 9c, with very low O_2 and N_2O?

Answer:

In this section, we compare the averaged December month over 8 years with the first N_2O *in situ* data collected during December 2009 in the Namibian upwelling system. We cannot compare the model and the data on a point-by-point basis as the model is forced with climatological monthly fields and not the exact forcing for December 2009. So using this climatological simulation, we only want to compare the order of magnitude, the spatial distribution (Fig. 9a and 9b), and the N_2O concentrations as a function of O_2 (Fig. 9c) using the first *in situ* N_2O data.

Yes, the model produces much higher N_2O concentrations (> 50 $10e^{-3}$ mmol N_2O m^{-3}) at low O_2 concentrations (but still > 10 mmol O_2 m^{-3}) than observed. However this increase in the N_2O yield at low oxygen concentrations is well described in the literature (i.e. Jin and Gruber, 2003). Two regimes of N_2O production are known: one pathway is associated with the nitrification process; the second pathway occurs at low oxygen concentrations and results in an interaction between nitrification and denitrification reactions. This second pathway generates a strong stimulation in N_2O production. At very low O_2 concentrations (below a few mmol O_2 m^{-3}), the situation is more complex as N_2O gets consumed. This extreme situation cannot be reproduced by the model, with very low O_2 and N_2O concentrations. The current N_2O parameterization from Suntharalingham et al. (2000, 2012) does not take into account the N_2O consumption at very low O_2 concentrations. We commented on this in the submitted paper "Finally, we do not at present explicitly account for the consumption of N_2O during the second step of denitrification as the parameterization of N_2O production of Suntharalingam et al. (2000, 2012) is only 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) or maybe up to 10 mmol O_2 m^{-3} as reported by Zamora et al. (2012) in the Eastern Tropical Pacific. However, N_2O consumption is not a relevant process in our Reference simulation; oxygen minimum values usually remained above the threshold limit of 10 mmol O_2 m^{-3} (see 3-day averaged oxygen concentrations in Fig. 9c). We will include this consumption process using the formulation of Jin and Gruber (2003) or Zamora et al. (2012) in future work." This comment now situates lines 1013-1021 in the revised paper.

Lastly, in situ data are scarce off Namibia, and it is therefore difficult to evaluate their significance. There are only two points with very low N_2O and O_2 concentrations. For one of the two, there is only one measurement; the second one is a mean over 3 measurements. So we need more N_2O data at very low O_2 concentrations in this area to really conclude.

section 4.2, line 16 It may be worth explaining that this solution may act on the fluxes and that if they keep balanced, it will not modify the concentrations. The question is: does the improvement about the fluxes also improve the concentrations of the various chemical species?

Answer:
In the sensitivity analysis, we were interested in improving the concentrations of the main biogeochemical variables (oxygen, nitrate and Chl-*a*) as well as the relevant fluxes of the nitrogen cycle (especially nitrification, denitrification and anammox processes). At Test 4 (increased vertical velocity and decreased decomposition rates of detritus and DON), the simulated oxygen, nitrate and Chl-*a* concentrations are satisfying as compared to the data. So in the following tests, we tried to improve the nitrogen fluxes as well as the N_2O vertical profile without deterioration of the distribution of oxygen, nitrate and Chl-*a* concentrations. We tried to find a good compromise to best satisfy our objectives.

The anammox rate alone has little impact on microbial fluxes (Tests 6 and 7). Nevertheless, combining the anammox rate increase with a reduction of the nitrification rate (Test n°14) clearly improves the anammox fluxes off Namibia. Indeed, NO_2^- is the limiting factor for anammox bacteria. This combination does however deteriorate the N_2O vertical profile linked with the OMZ as nitrification directly affects the N_2O production. So, we looked for an indirect way to increase the NO_2^- pool in the OMZ and thus anammox fluxes without changing the nitrification rates. NO_3^- reduction rate represents another way to increase the NO_2^- pool in the OMZ (Test n°9). Combining the relevant tested parameters, the Reference simulation (Test n°15) does not affect the distribution of oxygen, nitrate and Chl-a concentrations and gives satisfying results for N_2O vertical profile as well as nitrogen fluxes (NH_4^+ and NO_2^- oxidations, NO_3^- reduction and anammox processes) within the simulated OMZ off Namibia.

In the revised version of the paper, we changed “So we preferred to improve the different fluxes considered here without changing the nitrification rates. Another way to improve the nitrification fluxes in the OMZ comes from the significant source of NO_2^- by NO_3^- reduction and NH_4^+ by suboxic decomposition processes (Test n°9; Fig. 11). Moreover….to data” to “Thus, we looked for an indirect way to increase the NO_2^- pool in the OMZ and thus anammox fluxes without changing the nitrification rates. An NO_3^- reduction rate increase represents the other way to increase the NO_2^- pool in the OMZ (Test n°9). lines 771-773.

In conclusion, the improvement of the fluxes also improves the N_2O concentrations, without significantly changing the oxygen, nitrate and Chl-*a* fields.

section 5.1 p15085, lines 11-14 : please make clearer the volumes achieved in the model compared to the data. Is it really relevant to mention a factor of 65.9 when starting from almost nothing?

Answer:
We agree and changed these sentences. In the revised version (lines 845-851), we included the OMZ volumes explicitly: “For example, the OMZ was almost inexistent when starting the sensitivity analyses (Test 1 in Fig. 13) and became noticeable in the Standard simulation (0.8 10^2 km^3; see Test n°4 in Fig. 13). The OMZ volume is still doubled at the end of sensitivity analyses (1.7 10^2 km^3; see Test n°15 in Fig. 13) but differs by a factor of 2.3 from the CARS database (3.9 10^2 km^3). However, this volume difference comes from the area shoreward of 130 m isobath. Excluding this area, both estimations are very close (1.7 and 1.8 10^2 km^3 for the model and CARS, respectively; Fig. 13).”

In Figure 13, most of the error bars for hypoxic water volumes reach very low values. Does this come from inter-annual variability or from a trend in the development of the OMZ along the 8-years of simulations considered ? In both cases, it is questioning. If this is a trend, I find it problematic for the validity of the interpretation of your sensitivity analysis. If it is inter-annual variability, this is hughe and would be worth commenting where such fluctuations are coming from.

Answer:
You mean very high standard deviations? If so, yes, it comes from the internal or intrinsic variability of the ocean using an eddy-resolving coupled model. Non-linear processes introduce a huge variability, even using a climatological forcing. It is not a trend over the 8-years. These standard deviations are high for hypoxic water volume as well as for suboxic water volume (Fig. 13). We added a sentence on this feature in the revised version of our paper. “The standard deviation of the hypoxic and suboxic volumes over the 8-year of simulation is sizeable due to the intrinsic variability of the ocean using an eddy-resolving coupled model”. (lines 827-828).

In section 5.3, I am not sure whether the N2O production budgets over the studied area are relevant, since part of the shelf area (shoreward of 130m isobath) is excluded from budgets because of a lack of oxygen depletion. This could be discussed more precisely.

Answer:
We agree and had added some caveats to our conclusions. We mention in the revised version of the manuscript that this budget underestimates the N_2O production inshore line of 130-m depth as the OMZ is not correctly reproduced on the continental shelf (see lines 986-987). “Our budgets especially underestimate the N_2O production inshore of 130-m depth as the OMZ is not correctly reproduced on the continental shelf.”

section 5.3 The authors could mention potential sources for parameter improvements, like derived proxies for existing communities (e.g. ladderanes).

Answer:
We agree. We added in this section, lines 1009-1012 of the revised paper “So more studies, especially derived proxies for existing communities (e.g. ladderanes as in Kuypers et al., 2005) are needed to better understand....”. We have already used the few anammox data (rates,..) from Kuypers et al., (20005) and Lavik et al. (2009) estimated using N15-isotopes and ladderanes to trace anammox bacteria.

Technical corrections

p15054, line 20: Please make clearer what kind of model you are referring to, and what you call bias.
Answer: In the revised paper, we specified the type of model and bias (lines 73-78). The global models represent the Global Climate Atmosphere-Ocean models used in the CMIP5. The bias here is the difference between the mean over the different CMIP5 models and the mean of the observations.
“Currently, these EBUS are crudely represented in the Global Climate Atmosphere-Ocean models used within the Coupled Model Inter-comparison Project 5 (CMIP5) due to their coarse resolution. A

mean warm bias of 2-3°C (difference between the mean over the different CMIP5 models and the mean of the observations) is estimated for these models (Toniazzo and Woolnough, 2013).”

p15054, lines 22-23: Any reference for these expected changes?
Answer: We added two references: Garreaud and Falvey (2008) for the wind change and Bakun et al. (2010) for the productivity change.

p15056, lines 4-6. Over which period of time is this trend observed?
Answer: We added this information in our revised paper (lines 114-117). This SST trend is observed over the SST satellite data period from 1997 up to 2006-2007.

*p15061, line 18. In equation (18), f’ is formally also dependent on NH4+: f’Pi(NO3-,NO2-,*NH4+)
Answer: We agree and made this change in the revised paper (line 246, equation(18)).

p15062, line 10. Please correct : "...phytoplankton is not limited..."
Answer: We made this correction (Line 257) in the revised paper.

p15062, equation (19). Replace "avec" with "with".
Answer: We made this correction (Line 263) in the revised paper.

p15070, lines 20-27. Please rephrase and indicate which symbols of the graph you are refering to.
Answer: We rephrased and indicated now the symbols for the comparison between the model ouputs and the CARS climatology (green and red symbols) in the revised paper. (lines 454-456)

p15070, line 29 and p15071 lines 1-2. It seems the terms "spring" and "autumn" are inversed as compared to the values reported in Table 3. Please check.
Answer: Sorry, there was a mistake in Table 3; the text and the interpretation were however correct.

p15075, line 9. I suggest to rephrase as follow: "However, the amplitude between the extremes ... is lower..."
Answer: We rephrased accordingly in the revised paper (lines 578-580).

p15084, line 20. The reference (Hofmann et al., 2011) is not in the references
Answer: Sorry, we added this reference and checked the other references.

p15085, line 22. I guess you mean "to reduce NO3- and NO2- to NH4+" ?
Answer: No, we were talking about the nitrification process. We made a mistake. Thus we changed “…a maximum of nitrifying activity (that uses O_2 to oxidize NO_3^- and NO_2^- to NH_4^+)…” to “…a maximum of nitrifying activity (that uses O_2 to oxidize NH_4^+ and NO_2^- to NO_3^-) …” in the revised paper (lines 856-858).

p15086, line 2. replace "expect" with "except"
Answer: We made this change in the revised paper (line 864).

p15087, line 14. Do you mean Fig14f instead of 14e ? Indeed only the biogeochemistry consumption induces a oxygen sink.
Answer: Yes, you are right. We removed "(Fig. 14e)" in the revised paper (lines 900 902) as we already refer to Fig. 14f in the same sentence.

p15089, line 13. As no data are shown in Fig12a, it is difficult to tell that there is improvement in the mean N2O concentration. Please rephrase the next sentence: replace "This improvement provides maximum N2O..." with "This increase provides improvement in maximum N2O..."
Answer: We made this proposed change in the revised paper (lines 968-970).

Caption of Table 6. Please add "(test 15)" after "reference simulation"
Answer: We added this information in the caption of Table 6 in the revised paper.

Caption of Fig. 14. Do you mean that fluxes are computed at the depth of minimum oxygen concentration ?
Answer: Yes, we rephrased the caption of Fig.14 accordingly.

Report #2

Anonymous Referee #2

General Comments

1) The paper by Gutknecht et al. describes a numerical physical and biogeochemical model of the Benguela Upwelling system and validates it based on available observation data. A companion paper (Gutknecht et al., 2011; which I do not know) apparently uses this model to elucidate the N-cycle in the oxygen-poor environment, with a specific focus on N_2O production and various processes eliminating reactive N. This current paper appears to be the "methods section" of the predecessor paper, although here significant emphasis is put on modelling the N-cycle as well. The paper is very technical and has no question apparent to me which it attempts to answer. Half of the text is devoted to describing, tuning and sensitivity testing the model. Not being a modeller, I cannot comment on the correctness of the assumptions and formulations that have gone into the model. But I asked myself who would find this interesting: Is the description and tuning of a model suitable as the content of a scientific paper? Is this paper written for other modelers, and is the added work that the authors have invested into improving the previous versions of ROMS and BioEBUS significant? Can the paper be published as a technical note? But I asked myself who would find this interesting: Is the description and tuning of a model suitable as the content of a scientific paper? Is this paper written for other modelers, and is the added work that the authors have invested into improving the previous versions of ROMS and BioEBUS significant? Can the paper be published as a technical note? I see from the BGD web page that this is a contribution to a special issue on "Low oxygen in marine environments from the Cretaceous to the present ocean: driving mechanisms, impact, recovery". It is difficult to see how this manuscript contributes to this: There are no independent insights to be had from the paper, except that "more studies are needed to better understand the N cycle and improve its representation in biogeochemical models".

Answer:

It is the first attempt (in a published paper) to use a 3-D coupled physical/biogeochemical model at high resolution in the Benguela upwelling system, taking into account specific processes linked with the OMZ (denitrification, anammox and O_2 dependence on nitrification). In this submitted paper for the special issue, we describe for the first time this new biogeochemical model for the Eastern Boundary Upwelling Systems (BioEBUS) taking into account the relevant processes associated with the OMZ in Section 2. Then we evaluate its performance as compared to the data in Section 3, including the first N_2O data in this area collected during the FRS Africana Cruise in December 2009 in the framework of the GENUS German project. This exercise is a crucial element of any rigorous modelling study and has to appear in a published scientific paper. In Section 4, we present a sensitivity analysis performed on key biogeochemical parameters and the N_2O parameterisation. It is essential to evaluate our representation of the OMZ and associated processes as a function of these parameter values. Finally we present the impact of these key parameters on the OMZ volume, the nitrification, denitrification and anammox processes as well as the coupled physical/biogeochemical processes at work maintaining the OMZ in Section 5.

In the revised version of our paper, we explicitly mentioned our scientific questions as recommended (Lines 132-137).

"In this paper, we will especially address the following questions, after an evaluation of the model performance: What are the key parameters of the biogeochemical model and their influence on the OMZ representation, N losses due to denitrification and anammox processes, and N_2O concentrations and emissions to the atmosphere? What is the relative importance of the different coupled processes maintaining the OMZ in the northern part of the Benguela upwelling system?"

In another companion paper in revision for BG (Nitrogen transfers off Walvis Bay: a 3-D coupled physical/biogeochemical modeling approach in the Namibia upwelling system" by Gutknecht et al.), we used our best set of biogeochemical parameter values (Reference simulation) deduced from our sensitivity analysis (discussed in the paper for the special issue) to evaluate:

- the role of eddy mesoscale activity on the nitrate distribution in the Benguela Upwelling System.
- the full nitrogen budget (physical and biogeochemical processes) in the mixed layer with an estimation of the N-offshore export at 10°E and the N-export at the mixed layer base taking into account the N-loss associated with the OMZ.
- an estimation of the Benguela upwelling system as a N_2O source for the atmosphere.

We think our paper is especially suitable for this special issue *on "Low oxygen in marine environments from the Cretaceous to the present ocean: driving mechanisms, impact, recovery"*. Our study quantifies specific biogeochemical processes occurring in OMZs, including detailed analysis of OMZ volume, nitrification, denitrification and anammox processes. We discuss the relative importance of the coupled physical/biogeochemical processes at work in maintaining the OMZ within the Benguela upwelling system between 19 and 28.5°S.

2) As it is written now, it is very long, often awkward in its formulations ("In the OMZ off Namibia, N2O emissions to the atmosphere are comparable with N loss" – do you mean quantitatively?) or outright enigmatic ("Thus, other more classical variables are
also necessary as nitrites") and generally a difficult read for a non-specialist.

Answer:

As recommended, we rephrased these sentences in the revised version of our manuscript.: "Namibian OMZ" lines 55 and elsewhere; "the magnitude of N_2O outgasing and of N loss are comparable" line 55 .

We also made other modifications for an improved rephrasing of the text : lines 73-78; lines 771-773; lines 827-828; lines 905-910; lines 911-918; lines 986-987; lines 1009-1012; lines 1013-1021.

3) My advice would be that the authors consider cutting short much of the model descriptions and tuning, and concentrate on 1 or 2 questions that they can address with the model in experimental mode and that observations are likely unable to answer: For example, what is the quantitative role of the OMZ in eliminating reactive N over a typical year? Is denitrification indeed balanced by nitrification if integrating over the entire system?

Answer:

In any modelling work, the first time a model is used, the authors have to describe this model, such that other scientists in this field may reproduce the results presented. Thus in our paper for this special issue on O_2, we describe the BioEBUS biogeochemical model taking into account specific O_2-dependent processes associated with OMZs. The first reviewer found this description interesting and helpful.

If a model is not validated, then there are no grounds for believing its predictions; and if model sensitivity analysis is not performed, we cannot assess the uncertainty in predicted fluxes. The tuning/sensitivity analysis of the model is an essential precursor if the model is to be subsequently used to address questions about OMZs and nitrogen budgets. Such analyses need to be made public and open to scrutiny in scientific papers, given the importance of the questions that a validated model could help to answer concerning OMZs and the fact that many of these cannot be addressed with direct observations.

In our paper, after the model description, model validation and sensitivity analysis, we were able to put forward the key parameters of the biogeochemical model and their influence on the OMZ representation, N losses due to denitrification and anammox processes, and N_2O concentrations and outgasing and the relative importance of the different coupled processes maintaining the OMZ in the northern part of the Benguela upwelling system.

Specific questions on the Nitrogen cycle in the OMZ of the Northern Benguela upwelling system (i.e. N loss over a year) are addressed in a companion paper in revision for BG as explained in our answer to the first question.