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Biogeosciences Discussions

Interactive comment on "Controlling factors of the OMZ in the Arabian Sea" *by* L. Resplandy et al.

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A serious consequence of global warming that is increasingly gaining importance is the issue of ocean deoxygenation and its impacts on ocean productivity, nutrient cycling, carbon cycling and marine habitats for higher trophic levels. Current models exhibit severe biases in simulating both vertical and horizontal oxygen distribution. In particular, the establishment and maintenance of the OMZs, as well as their variability associated with a wide range of spatio-temporal scales, remain unresolved issues. In that respect, the study by Resplandy et al. concerning the Arabian Sea (AS hereafter) OMZ, one of the most intriguing OMZs, is very welcome. The authors propose here to elucidate two issues: the lack of seasonality in the observed OMZ structure in the AS and the spatial offset between the core of the OMZ (located in the northeast AS) and the highly productive region (located along the western coast). My main concern is the lack of proper validation of the modelling tool used by the authors to build their conclusions.

Indeed Figures 1 and 3 speak for themselves. Modelled chlorophyll a concentrations do not mimick the observed ones in the whole AS region and in CAS and OMA subregions during both the Northeast and South west monsoon regimes. Only the IND subregion behaves quite adequately. I went to consult the referenced article from Resplandy et al (2011) to check whether the nutrients (nitrate) fields were represented with some realism. Figures 1 and 2 of this latter reference clearly show that this is not the case both in nitrate levels (one order of magnitude difference in some locations between model outputs and observations) and spatial distribution patterns during both monsoon regimes. The authors should dramatically improve the realism of their simulations and provide a rigorous skill assessment of the model with metrics of goodness of fit (contingency tables, Taylor diagrams, wavelet analysis, see for instance Saux Picart et al., 2012, . . .) to observations for the major biogeochemical properties: nitrate, chlorophyll a, and oxygen concentrations (both zonal and vertical sections for these properties) in the AS and in subregions CAS, OMA and IND. Unless this is done, the paper cannot be accepted for publication in Biogeosciences. In the present state, any inference on the relative importance of the physical (ventilation) versus biological (consumption/production) processes which might control the seasonality and establishment of the OMZ cannot be considered with confidence.

We agree that our model presents some biases, which are presented in the manuscript. We also agree that the job performed by biogeochemical models is usually less satisfying than the one of physical models. However, we strongly disagree with the reviewer view that the model does not mimick the observed chlorophyll features. In the following we attempt to show what the state of the art in biogeochemical modeling in the Indian Ocean is. Figures showing simulated and satellite chlorophyll fields in two of the most recent and realistic modeling studies (Kawamiya and Oschlies, 2001 on Fig 1; Wiggert et al. 2006 on Fig 2) show that models systematically tend to underestimate the blooms in the central Arabians Sea. One of the hypotheses is that models do not represent mesoscale that are known to be of primary importance in that region. For that reason, in

our 2011 study (Resplandy et al., 2011), a biophysical model of the Indian Ocean resolving the mesoscale was used. It allowed to reproduce the spatial variability in the physical field (Fig 4 in Resplandy et al., 2011) and the mesoscale features that structure the two seasonal bloom (Fig 3 below, adapted from Resplandy et al., 2011).
We agree that the nitrate levels are not perfect in our model. This is a recurrent bias of all biogeochemical models in the region as illustrated in the comparison of nitrate concentrations in the study of McCreary et al. (submitted) provided in Fig. 4 below. One of the major reason for that is that vertical resolution in biogeochemical models does not allow the representation of the strong gradients observed in situ. Another source of bias is introduced by the fact that the model is of course a simplification of the real world: not all processes are taken into account and consequently not all parameters can be perfect. We strongly disagree with this view that only a perfect model, which does not exist because it would include all processes that we don't know about, can be considered. Indeed, we learn as much from the biases of our models (highlighting processes that are not yet understood) than from the points that are 'mimicking' the real ocean. The reviewer should also consider the fact that if we knew how to correct all our biases we would indeed do it before submitting (a modeler dream). I guess it is the same for observations, we dream of unbiased observations with no spatial and temporal undersampling. However we have to deal with the fact that we, oceanographers, don't understand all the processes at play. I also would like to stress out that the effort put into our model to represent the mesoscale largely improved the representation of the nutrients and the chlorophyll compared to the state of the art in the region.

Minor comments:

Page 5511: Line 5: I would not say that one can find the most intense OMZs in the Eastern tropical Atlantic. **We agree and therefore changed this sentence.**

Page 5512: Looking carefully at Figures 1d and 2d, one see a seasonal difference in the spatial extension of the very low oxygen concentration tongue between the Northeast (NEM) and Southwest (SWM) monsoon regimes. During NEM, the low tongue is centered along 20 ÌŁN until 62 ÌŁE, in SWM, it occupies a much larger volume north of 20 ÌŁN and is oriented southeast-northwest. Actually the model is not able to reproduce these low O2 tongues (Figures 1e and 2e).

We agree that waters with very low oxygen values (below 20 micromol/L or core waters) are not enough represented in the model. This was mentioned in section 2 and has been re-written for more clarity (see paragraph below). However, the reader should keep in mind that seasonal changes in the shape of the low oxygen tongue in observations may arise from the undersampling bias in the climatology. It is indeed not very likely for the core surface (<20 micromol/L) to be divided by two between the NEM and the SWM (see Fig 5 provided here). This figure comparing the core volume in model and observations has been added to the manuscript as advised by the reviewer in a comment below. This is again one of the strength of using both observations and models (even if they are not perfect) in combination. "The main characteristics of the OMZ are reproduced by the model. Oxygen concentrations are however slightly larger than observed in the upper OMZ (âĹij100 m depth) and along the west coast of the Arabian Sea inducing a reduction of the core's volume and an overestimation of the eastward shift of the OMZ (Fig. 1 panels g-j and Fig. 2). This bias primarily arises from the relatively low vertical resolution in the model (46 vertical levels with 10 levels between 50 and 300 m) that is insufficient to properly resolve the sharp oxygen gradient of the oxycline (Fig. 2). Along the west coast of the Arabian Sea oxygen concentrations between 400 and 1000 m are $\geq$30 μmol.L$-$1 in the model, whereas concentrations $\leq$20 μmol.L$-$1 are observed (see contours on Fig. 2 a,b). This overestimation of the eastward shift in the model however confirms that the this feature is simulated and maintained in the model and is not only arising from initial conditions."

Line 23: .. from the compensation Line 24: ..compensation.. **Ok thank you.**

Page 5513: Lines 10-12: Lam et al. 2011 showed that both anammox and denitrification genes were abundant in the AS so this sentence should be modified accordingly. **This has been modified.**

Page 5514 : Line 24 : ..dinoflagellates Page 5515: Line 20: How the f(O2) function was determined? Any physiological basis? What is (O2)?

The paragraph on this formulation was unclear. It has been re-written as follows: "A key process in modulating oxygen concentration in the model is the remineralization of DOC. It can be either oxic or anoxic depending on the local oxygen concentration. The splitting between the two types of organic matter degradation is performed using the factor f(O2) comprised between 0 and 1:

$$f(O_2) = 1 - \min\left[1, 0.4\frac{\max[0,(6-O_2)]}{1+O_2}\right]. \quad (1)$$

When $O_2 > 6\mu$mol.L^{-1} ($f(O_2)$ = 1), remineralisation is striclty aerobic and only consumes oxygen. However, when $O_2 < 6\mu$mol.L^{-1} ($f(O_2) < 1$), part of the organic matter remineralisation consumes nitrate instead of oxygen (denitrification). Implicitly, degradation rates for respiration and denitrification are therefore identical."

Page 5516 : Lines 24 to 5-page 5517: One reads that the model is 1/12 ÌŁ resolution and then the authors explain they are considering a lower resolution (1/4 ÌŁ) version of the coupled model. It became clear only on page 5524 that this set up was devoted only to the perturbation experiment. I would advise to omit these lines in section 2.2. **The perturbation experiment was removed from the paper and only the main run at 1/12° is now used.**

Page 5517: Line 3: . . . intense as in the OMZ observations. **Thank you**

Page 5518: Figure 3 is a bit misleading, the authors should have chosen the same 20

μmol.l1 for both modelled and WOA oxygen levels. Along the EW and NS sections, the modelled oxyclines are much more diffuse than the observed ones. How are the modelled nitraclines? Did the authors try an increased vertical resolution in the top 300m to ensure the bias is due to the number of levels? If one considers the 20 μmol.l1 contour, the model underestimates the OMZ core volume (Figure 3a and b) west of 58 ÌŁE and the ultra low oxygen tongue lies above the continental shelf along the Indian coast in the model. How the authors can explain this discrepancy with observations? It would be interesting to provide an oxygen section along an EW section along 20 ÌŁN where the very OMZ core extends. A comparative plot of modelled and observed OMZ core volume and depth range of the OMZ core could also serve for a proper model skill assessment. The authors recognize some biases of the model (lines 16 to 23 page 5518 and lines 19 to 25 page 5519) but it of the utmost importance to correct them before making any quantification of the oxygen budget.

We think that it is important to show that the shape of the eastward shift is right in the model even if the absolute concentrations are not. This is why we showed different oxyclines on Fig 3. We agree that the model oxyclines are more diffused and we identified that as one of the major bias. We can't perform such sensitivity test in this configuration as it would mean a full simulation if we want to see if the bias is reduced and the computing costs is too high. The current simulation already represents month of computation. However, it is well established that increasing the vertical resolution allows a better representation of vertical gradients as large differences in concentrations can not be maintained for long in adjacent model cells. We plotted different sections before choosing the one at 15N because it shows a larger range of concentrations (including a large portion of the core in observations) and larger portion of the Arabian Sea (the basin being very narrow at 20N).
To adress the reviewer concern about the model validation, we made a figure comparing core volume (<20 micormol/L) and the volume of waters with oxygen concentrations lower than 60 micromol/L (see Fig. 5). As discussed in the paper,

the core volume is underestimated but the OMZ volume is correct. This figure will be included in the paper because it is true that it quantifies the model bias. Again we strongly disagree on the view that only perfect models, where all biases have been corrected can be looked at (see above).

Page 5519: One would rather use the oxycline instead of the "top of the OMZ" (lines 4, 6 and 10). **Indeed, we agree. This has been changed in the manuscript.**

Page 5520: Lines 10-11 : The authors find that the amplitude of the dynamical trend is by far larger than the biological contribution which does not support Sarma's (2002) results. How confident can we be in the model outputs? In addition, the model does not include some complexity of the nitrogen cycle (anammox for instance is not included) thereby impacting on nutrient fields which in turn impact on organic matter remineralization and consequently oxygen contents.

We would like to emphasize that the dynamical and the biological trends do compensate on annual average (otherwise the model would drift). However, we find that this is not true on a monthly basis. We would like to stress out that Sarma et al. (2002) does not prove that biological and dynamical trends compensate at each season, due to the bias on biological terms derived from observations. On the contrary, this compensation is the axiome he based is study on to equilibrate his oxygen budget. In addition, the large impact of the dynamical trend on the oxygen budget is supported by the results of McCreary et al. (submitted), who finds that the dynamical transport by the western boundary current is a major process in ventilating the eastern boundary. Indeed, anamox is not included in the model. However, I doubt that this process alone would compensate the dynamical input of oxygen by the Somali Current, in which the world fastest currents where observed. Again, we insist on the fact that on an annual basis, the biological and dynamical terms compensate.

Page 5521: Line 3: ..ventilated during the FIM and NEM . . . Page 5522: Line 11 ..that

strongly influence. . . **Thank you**

Lines 10 to 12: It would be nice to show that the oxygen vertical transport is indeed modulated in the IND box by the interaction between 2nd baroclinic mode Kelvin and Rossby waves. **We agree that some clarification was missing to understand this part. We therefore added a figure of alongshore and vertical currents that show the vertical propagation, which is characteristic of the propagation of 2nd baroclinic mode Kelvin waves in this region (Nethery and Shankar, Journal of Earth System Science, 2007)**.

Page 5523: Lines 21 to 23: . . . the presence of the OMZ is explained essentially by 1D processes. This statement is quite not true since in the OMZ off Peru or Namibia, the OMZ pres- ence results generally from a complex 3D balance between circulation and biological processes.

Of course we agree that 3D processes are at play. The "1d" term was not chosen with enough care. What we meant is that in most OMZ, biological production and OMZ are co-located in space at the basin scale (but not strictly in 1D of course). In contrast, this is not true in the Arabian Sea where the OMZ is shifted. We reformulate this sentence to clarify this point:
"In most OMZs, the lowest oxygen concentrations are found in regions of productive upwelling systems and weak subsurface currents that are poorly ventilated. The existence of the OMZ is therefore essentially explained by the degradation at depth of the organic material produced at the surface, which is the biological uptake of oxygen. In the case of the Arabian Sea, the OMZ is not found below the area of strongest productivity, that is, the upwelling region of Oman. Rather, it is shifted toward the center of the Arabian Sea, suggesting that dynamical transport plays a key role in the oxygen budget of this OMZ."

Page 5524: Lines 9-10: I think this is not the correct figure number. **Okay. Thank you.**

Lines 15-21: Why did the authors choose 100 μmol.l-1 to set their initial oxygen concentration? How sensitive are the results to this initial resetting of the OMZ? Pages 5526 : Lines 1 to 6: If one looks at figures 11- 1c, and 3c, low oxygen waters are already in the central eastern Arabian sea at the beginning of the perturbation simulation so I don't follow the argument of advection redistributing oxygen within the basin. Line 10: What do the authors mean by proto-OMZ? **To adress both reviewer concerns, the perturbation experiment was removed because of the problems inherent with the methodology.**

Page 5528: I cannot see on Figures 2 and 6 any influence of the propagation of coastal Kelvin waves and westward propagating Rossby waves. **The vertical propagation of a signal (here on all trends and currents) with time is a typical signature of the propagation of second-baroclinic mode waves. In addition, the propagation of second-baroclinic mode Kelvin waves has been identified as a key process in the region(Han et al., 2011; Nethery and Shankar, 2007)**

Interactive comment on Biogeosciences Discuss., 9, 5509, 2012.

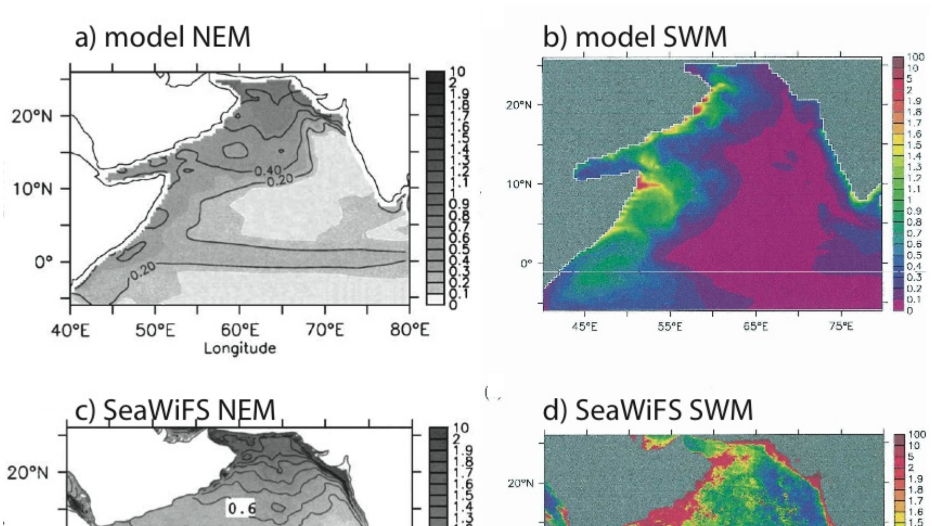

Fig. 1. Seasonal mean surface Chl in Kawamiya and Oschlies (2001) in model and satellite observations

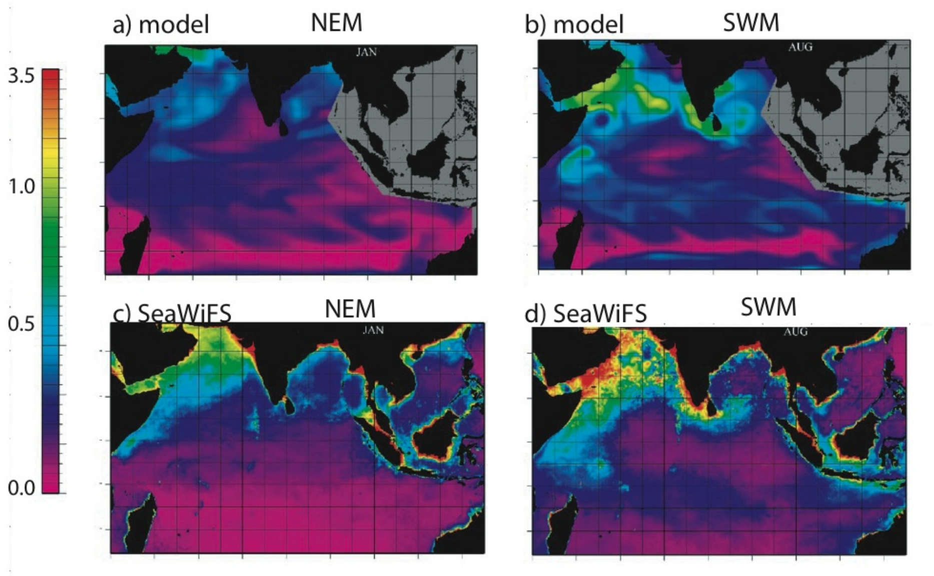

Fig. 2. Seasonal mean surface Chl in Wiggert et al. (2006) in model and satellite observations

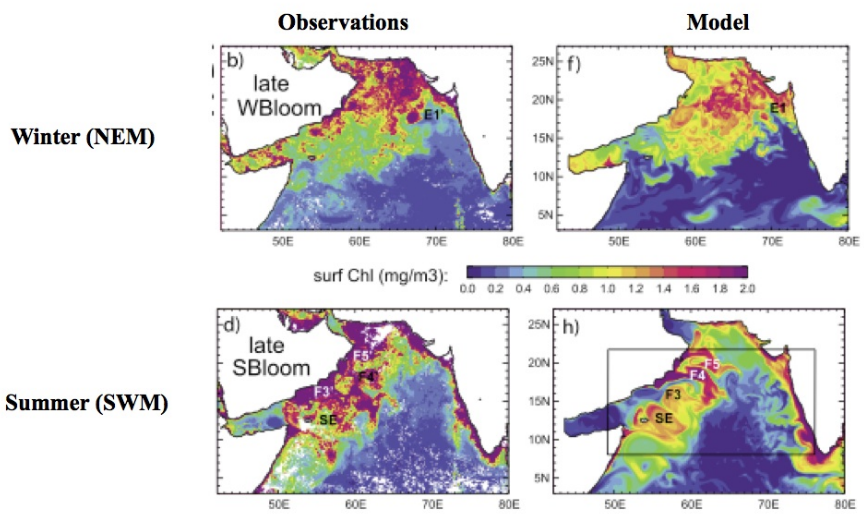

Fig. 3. Snapshots of surface Chl in our model (Resplandy et al., 2011) and satellite observations

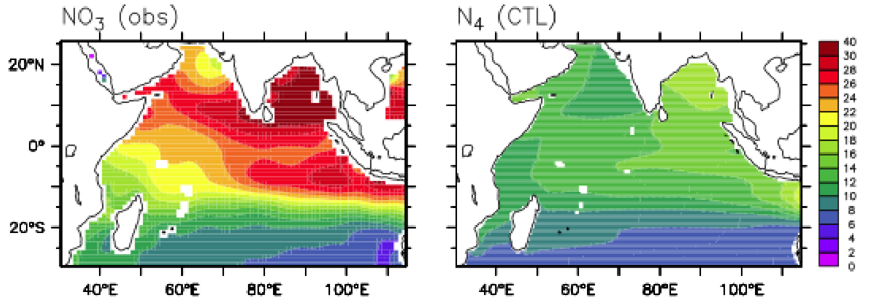

Figure 4b: Maps of nutrient concentrations on the 26.6 σ_θ density surface from WOA05 data (left) and the corresponding N_4 field from the control run (right).

Fig. 4. Nitrate in McCreary et al. (submitted) model and observations

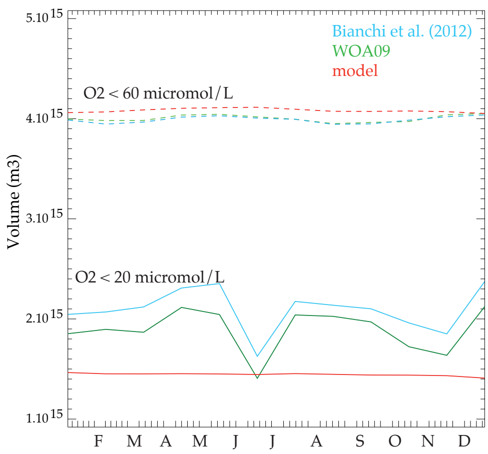

Fig. 5. Volume of waters with oxygen concentrations lower than 20 and 60 micromol/L in the model and 2 climatologies derived from observations