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Biogeosciences Discussions

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

Anonymous Referee #2

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General comments :

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.

Minor comments:

Page 5511: Line 5: I would not say that one can find the most intense OMZs in the Eastern tropical Atlantic.

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 North-east (NEM) and Southwest (SWM) monsoon regimes. During NEM, the low tongue is centered along 20˚N until 62˚E, in SWM, it occupies a much larger volume north of 20˚N and is oriented southeast-northwest. Actually the model is not able to reproduce these low O_2 tongues (Figures 1e and 2e).

Line 23: .. from the compensation

Line 24: ..compensation..

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.

Page 5514 : Line 24 : ..dinoflagellates

Page 5515: Line 20: How the $f(O_2)$ function was determined? Any physiological basis? What is Δ (O2)?

Page 5516 : Lines 24 to 5-page 5517: One reads that the model is 1/12˚ resolution and then the authors explain they are considering a lower resolution (1/4˚) 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.

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

Page 5518: Figure 3 is a bit misleading, the authors should have chosen the same 20 μmol.l^{-1} 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.l^{-1} contour, the model underestimates the OMZ core volume (Figure 3a and b) west of 58˚E 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˚N 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

BGD

9, C3364–C3368, 2012

correct them before making any quantification of the oxygen budget.

Page 5519: One would rather use the oxycline instead of the “top of the OMZ” (lines 4, 6 and 10).

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.

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

Page 5522: Line 11 ..that strongly influence. . .

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.

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 presence results generally from a complex 3D balance between circulation and biological processes.

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

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?

Page 5528: I cannot see on Figures 2 and 6 any influence of the propagation of coastal Kelvin waves and westward propagating Rossby waves.

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BGD

9, C3364–C3368, 2012

Interactive Comment