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# *Interactive comment on* "Effects of upwelling duration and phytoplankton growth regime on dissolved oxygen levels in an idealized Iberian Peninsula upwelling system" *by* João H. Bettencourt et al.

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Effects of upwelling duration and phytoplankton growth regime on dissolved oxygen levels in an idealized Iberian Peninsula upwelling system

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Discussion paper

Authors' reply to Referee #2

We thank the Referee for his/her comments that helped to greatly improve our manuscript. Below, we provide a point-by-point reply to all referee's comments.

Ref. #2: While I generally agree with the comments of Referee #1, in terms of the quality of the manuscript, and its relevance of publication, there are a number of points that I will refer to, which should be addressed before the final publication. On the one hand, the con- figuration used is highly idealized, and the practical application to the Iberian system is not straightforward. Provided the simplifications that were done related the idealized configuration, and the theoretical approach, (quite difficult to read to oceanographers not specialized in theoretical oceanography) I have some doubts about the practical applicability of the results, and the interest that it may have for the oceanographic community of the region, devoted to the study of biogeochemical processes, (constituted mainly by chemical oceanographers).

Authors: We acknowledge the concerns of Referee 2 concerning the future applicability of our results due to the theoretical character of our study. This contribution is indeed, to our knowledge, the first and unique attempt to develop a low complexity ocean coupled model ("NPZD-type", which has been proven very useful for the Oceanographic community) centered on oxygen by building upon recent mathematical modelling. To strengthen the utility and the potential applicability of our results, we have rewritten several parts of the manuscript to better summarize our results and make it easier for the oceanographic community to follow the theoretical description of our model (please check the revised appendix A).

Ref. #2: 1 - Introduction: Paragraph 1. The Iberian region is not an example of a region of marine hypoxia, and that should be referred to in the manuscript. References to deoxygenation (line 40) do not make much sense in the scope of this manuscript.

Authors: Following your recommendations, we have modified the Introduction section by removing the references to marine hypoxia and deoxygenation and by introducing additional references as suggested by the next comment. Both first paragraphs of the Introduction have been rewritten accordingly.

Ref. #2: 2 - The authors barely refer to the observational literature related to the oxygen cycle in the Iberian region, limiting themselves to cite Rossi's articles (2010, 2013), which consist of a single campaign, not particularly devoted to that subject. There is some observational literature including the oxygen cycle that I think should be cited, as it is relevant. (Hint: Alvarez-Salgado, X.A, Perez, F.F, Castro, C.G. , all of them in IIM-CSIC-Vigo, Spain) On the other hand there are oceanographic processes not solved by the model that influences the oxygen cycle, related to processes in the benthic layers, and on remineralization processes that influence the oxygen cycle, and that are not referred to.

Authors: We are grateful to the referee for bringing to our attention to this interesting body of previous research. We indeed referred to Rossi et al.'s articles since the dataset of this specific campaign was used to tune our model (e.g. nitrate/temperature relationship) and to compare its simulated outputs to in-situ observations. Note also that some of the authors suggested by referee 2 were already cited in our original manuscript (e.g. Alvarez-Salgado et al. 1993; Reboreda et al. 2014; 2015). Following his/her suggestions, we have now included additional references to Pérez et al (2001), Castro et al (2006) and Alvarez-Salgado et al. (2013) as follows:

"During the upwelling season, hydrographic and biogeochemical variability is primarily determined by the wind forcing that controls the inflow of offshore subsurface water masses onto the shelf (Alvarez-Salgado et al., 1993). DO levels in these subsurface waters are thought to diminish during the productive upwelling season due to strong remineralization of the sinking organic matter as well as to mixing with poorly-ventilated waters of subtropical origins (Castro et al., 2006). Overall, DO appears to partly control the remineralization of dissolved organic carbon in the IPUS and more generally in key water masses of the North Atlantic (Alvarez-Salgado et al. 2013)."

“These structures have a strong influence on the cross-shore transport and on the vertical redistribution of biogeochemical tracers (Bettencourt et al., 2017; Combes et al., 2013; Gruber et al., 2011; Hernández-Carrasco et al., 2014; Nagai et al., 2015; Renault et al., 2016; Rossi et al., 2013). In the Iberian basin, the Eastern North Atlantic Central Water (ENACW) mass of subtropical origin, whose archetypal concentrations of DO are initially low, would be ventilated by eddy-induced mixing with more oxygenated ENACW of subpolar origin (Pérez et al., 2001). .”

Concerning the second part of this comment, we agree with referee 2 that our O2PZ model disregards, as a result of its low complexity, some oceanographic processes that influence oxygen cycling over the shelf, such as complex processes occurring within the benthic boundary layers or into the upper sedimentary layers (i.e. the so-called benthic-pelagic coupling). We now acknowledged this limitation of our idealized modeling approach in the revised Discussion and Conclusions section (Section 4, 1st paragraph):

“We built a coupled physical-biogeochemical model of an idealized seasonal coastal upwelling to study the effect of the upwelling season length and phytoplankton community structure on dissolved oxygen inventory. The low-complexity O2PZ model accounts for oxygen production by photosynthesis and consumption by both respiration and remineralization but its simplicity does not allow to include other oceanographic processes that could influence oxygen cycling over shallow shelves, such as for instance the complex benthic-pelagic coupled processes.”

Ref. #2: 3 - Lines 62-64 need a reference that support those statement.

Authors: We have added appropriate references to support those statements.

“Moreover, diatoms growth is thought to be positively correlated with the upwelling-driven nitrate inputs (Sarthou et al., 2005); in contrast, the growth of other planktonic groups can be insensitive to and even limited by newly-upwelled nitrates (Mahaffey, 2005), especially when colimitation prevails or in the absence of necessary micronutrients.”

Ref. #2: 4 - Is the implementation of the Biogeochemical (BGQ) module done in CROCO model, or was implemented from scratch?. This is not clear in the ms. In that case, justify why none of the other BGQ modules was not used.

Authors: Our low-complexity model centered on oxygen was implemented from scratch in the CROCO model, i.e. it was hard-coded into the CROCO model following, as a template, one of the already-embedded BGQ modules. The O2PZ model was retained instead of other BGQ models available in CROCO because of its low complexity (only 3 equations) resulting in fast and efficient computing, hence allowing running/analyzing multiple simulations while considering the role of high-resolution dynamics (e.g. submesoscale) for oxygen cycling. It has been specified in the manuscript (beginning of sect. 2.1).

Ref. #2: 4- Is not clear the interaction with the atmosphere in the BGQ module, concerning O2 atmosphere - ocean exchange.

Authors: The air-sea fluxes of DO follow the formulation of Wanninkhof (1992). The flux is Va*([O2]-[O2]sat), where Va is the gas piston velocity, [O2] is the DO concentration at the water surface (the first sigma level below the surface) and [O2]sat is the O2 solubility in seawater. Please, see Equation 1 and the description of the DO gas exchange in the revised version of the manuscript (end of 2nd paragraph in page 4).

Ref. #2: 5- Laplacian mixing along sigma surfaces tends to generate spurious currents. Why you did not use (ro- tated) mixing along Geopotential?

Authors: The reason for not using mixing along Geopotentials was that this option is computationally heavier than Laplacian mixing along sigma surfaces, while their methodological biases are similar. We retained the option that resulted in a faster code without compromising the quality of our analyses.

Ref. #2: 6 - Is the wind only alongshore?, please specify.(l 109).

Authors: Yes. In our idealized setting the wind is only alongshore. Given the idealization level, we didn't see the need to include an across-shore wind component. We modified the text immediately above Table 1 to make this explicit:

"The alongshore wind profiles (Fig. 3(c)) are based on the cyclic wind pulses that are characteristic of the summer / early fall upwelling favorable conditions in the IPUS: 10-day pulses with maximum wind speed of 12 ms-1 (Rossi et al., 2013; Torres et al., 2003a). The cross-shore wind component is zero in all cases."

Ref. #2: 7 - It should be clearer to the reader why nutrients are not introduced specifically, and what is the advantage of doing it in an O2PZ model instead of O2NPZ?

Authors: The addition of a nutrient compartment to the O2PZ model would make it computationally heavier and we wanted to keep the calculations as simple and as fast as possible. Thus, given that the original formulation of the O2PZ model didn't have nutrient limitation mechanism, which is important for upwelling regions such as the IPUS. As density and nitrate are closely related, especially in upwelling regions, we came up with a relatively simple way to include this nutrient limitation effect in our oxygen centered model. Please, see the revised text in the last paragraph of page 4:

"The l(N) term is a Monod function l(N)=k1N()/(k2 + N()), where N() is a parametrization of nutrient availability based on the nitrate-density () relationships measured during an oceanographic cruise which surveyed the IPUS during upwelling season (the 2007 MOUTON campaign, see also Rossi et al. 2010; 2013). This term is included here to circumvent the absence of nutrient limitation in the original O2PZ model of SP2015. We chose to add a parametrization instead of a new equation for the nutrients because this last option would necessarily increase the complexity of the model."

Ref. #2: 8 - The 12m/s wind pulses although exist in particular years (like the MOUTON campaign) are too high and unrealistic for the Western Iberia region. Justify better why such high values. Some sensitivity studies was done to this value?

Authors: Our choice of the wind pulse was based on forcing the model with a wind whose magnitude is comparable (i.e. the same order of magnitude, bearing in mind that our configuration is highly idealized) to the one observed during the MOUTON campaign. We also retained the highest magnitude (rather than the mean or lowest) because it stimulates the quickest and clearest formation of the unstable upwelling front. Note however that preliminary analyses were carried out to test several wind speeds (not shown); we found that the model responses were qualitatively similar, provided that the magnitude used is above a certain threshold, ensuring the development of upwelling (e.g. isopycnal outcropping and subsequent front destabilization). Given those results, we decided to use the wind speed that represented best the MOUTON campaign forcing conditions. We have clarified our rationale behind these choices in sect. 2.2 of the revised manuscript.

Ref. #2: 9- I don't understand why in the initial vertical profile, (Fig 3a) the 02 drops to zero when in reality the O2 at 300m is closer to 230 mmol O / m3. The range of values in the region above central waters is 220-290 mmol O / m3.

Authors: The drop in O2 levels is a consequence of our very idealized setting. Indeed, as the referee rightly points out, O2 levels in the region at 300 meters are higher. However, in our highly idealized setting, we do not have subsurface O2 inputs due to the periodic boundary conditions at the North and South boundaries. Since in the O2PZ model, the only O2 source is photosynthesis, that is zero below the euphotic zone, without the subsurface O2 inputs, the subsurface O2 becomes depleted by respiration and remineralization. Note that this point (absence of subsurface O2 input) was already reported as an important point of discussion that we clearly identified as key for future work (please see sect. 4).

Ref. #2: 10 - Claim that figure 4 represents a validation seems too optimistic to me. I can't understand what the red dot represents in the figures.

Authors: Please, see the reply to point 11 below. The red dots are used to mark the position of the barycenter, i.e. the average values of the data for each quantity represented in the scatter plots, i.e. the x-coordinate of the red dot in figure 4a is the averaged density and the y-coordinate is the mean [O2].

Ref. #2: 11 - Using logarithms in Chla completely distorts the comparison, because the observations are showing values of 0-10 mg Chla.m-3 between isopycn 26-27 as the model rarely goes beyond 1 (at isopycn 27). Similar comment arises for O2. If you claim those results are validated, then, any result is validated. If you insist in keeping this part as it is, I suggest that you remove the word validation, perhaps use 'qualitative comparison'.

Authors: We agree that the use of logarithmic scales provides mainly a qualitative comparison between model results and observations; note however that Chlo-a concentrations are commonly plotted using logarithmic scales as values often span several order of magnitude, as is the case here. Nevertheless, we have modified the text to make this clear, removing all references to validation. Please, see the revised text of the last paragraph of section 2.

"We compared the full ranges of densities , DO concentrations [O2], and chlorophyll-a concentrations [Chl-a] of the reference simulation to all compiled measurements collected during the MOUTON campaign. Given the low-complexity of the biogeochemical model and the highly idealized physical setting, a fully quantitative comparison is out of scope, however we confirm that the coupled model reproduces qualitatively well the density-[O2] and density-[Chl-a] relationships obtained from in-situ observations."

Ref. #2: 12-Figure 5, I can understand the effect of upwelling at x= 40km that is related to shelf upwelling, but how do you interpret the dome at 60-70 km (approximately)? To use the references (l 200-204) to validate the averages of O2 seems to me to be an over-optimistic approach. Again you can say that it is a simplified and idealized model, but do not try to convince the reader that it is validated, because readers with observational background would hardly understand it.

Authors: We believe the dome at 60-70 km is caused by the downwelling signal induced by the eddy induced circulation. More specifically, this downwelling divides the region of recently upwelled low DO waters into (i) the inshore low DO region (within 50 km from the shores) and (ii) the dome located at 60 – 70 km, slightly off the shelf edge. We have modified the text to highlight that our results are similar in a qualitative sense. The revised text (1st paragraph of section 3.1) now reads:

"In a qualitative sense our idealized model results are similar to more realistic model studies such as , Gutknecht et al. (2013) which in their modelling study of the Benguela upwelling region found a low [O2] plume for the climatological month of December at the shelf edge, and to observational studies such as Hales et al. (2006), which for the Oregon coast, measured [O2] of 70-110 mmol m-3 in upwelled water at the shelf break, about 200 mmol m-3 less than at the surface, which is the same range of the vertical gradient simulated here."

Ref. #2: 13- Lines 210 - 215 seem more like a discussion, and their presence is not understood there.

Authors: Lines 210-215 provide a necessary, yet short, physical description of the simulation dynamics. It is needed to put the average DO distribution in the context of upwelling dynamics (at very first order, any oceanic tracer is influenced by the ocean circulation). Particularly, we felt that a discussion of the mean and the eddy-induced circulation was required to better explain the patterns of nearshore DO seen in Figure 5 (see also the first part of our reply to point 12: the "dome" is well explained by the eddy induced circulation which has been described just before).

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