We sincerely thank the reviewer for this excellent review and the constructive comments that largely contributed to the improvement of our manuscript.

In order to address the reviewer comments, major changes were made:

- **The perturbation experiment that has not yet reached equilibrium, which makes it interpretation hazardous, was removed from the manuscript.**
- **The spatial distribution of the OMZ in now described focusing on the main run. The drift of this run is now extensively discussed before focusing on the biological and dynamical trends in oxygen. The spatial distribution of mean, eddy-driven, vertical and horizontal components of the oxygen transport are examined.**
- **In the part focusing on seasonality, we clarified some aspects by adding figures of the horizontal circulation.**
- **Following the reviewer advice, we clarified the definition of the OMZ (core, oxycline, upper and lower OMZ).**

Specific answers to each of the comments and concerns (regular font) **are provided below (bold font).**

Review by Jay McCreary of manuscript no. bg-2012-138, entitled "Controlling factors of the OMZ in the Arabian Sea," by Laure Resplandy and coauthors

Page numbers and line numbers refer to the version of the manuscript that I downloaded from the Biogeosciences website.

1) page 5510, line 8: In the abstract you wrote: "We find that the oxygen concentration in the OMZ displays a seasonal cycle with an amplitude of 5—15% of the annual mean oxygen concentration." I think you should state here that the 15% occurs at the very top of the OMZ, when the oxycline is shifted up and down by ocean dynamics. The 5% occurs everywhere else in the OMZ. Right? That is, it is not the OMZ itself that varies by 15%, but only the depth of the oxycline.

We agree that the definition of OMZ upper and lower part was unclear. The text was modified as follows:

"As expected, the seasonality of the oxygen concentration in the OMZ is weak with a variability of the order of 15% of the annual mean oxygen concentration in the oxycline and 5% elsewhere. This weak seasonality results from an imbalance between oxygen advection and biological consumption on seasonal scale."

Furthermore, is it really correct to say that the OMZ is "ventilated"? To me that process means that oxygen is injected into the OMZ and therefore increases oxygen throughout its vertical extent. In fact, there is no (or little) ventilation at all, with only the oxycline shifting up and down.

The word ventilation was replaced by oxygen supply, advection, transport or similar expressions throughout the text.

2) page 5510, lines 23—24: I don't think you ever showed in the manuscript that low oxygen waters from the western AS actually advect eastward to cause the ASOMZ there. If not, then this last sentence should be replaced.

We agree that the diagnostic on the spatial distribution were not strong enough, mostly because the perturbation experiment had not reached equilibrium. The result section and abstract on this point has been re-written using the main run to perform the diagnostics. This sentence has therefore been removed.

3) page 5513, lines 1—15: I found this paragraph a bit confusing. The opening sentence is the first place where you note the eastward shift. I expected some further discussion of its particular aspects. The remainder of the paragraph, however, is mostly a discussion of denitrification, which seems o§

the main topic. You might try to expand the first sentence into a small paragraph. Then, have the discussion of denitrification in a separate one.

Following the reviewer advice, one paragraph is now dedicated to the spatial distribution topic.

4) page 5514, lines 20—21: What about horizontal mixing? This process should be very important in the dynamics of the deeper OMZ. Is it the same as in the biological model?

The advective scheme used in this study is diffusive. Horizontal mixing is therefore intrinsic to the advective scheme. This information was described in Resplandy et al. (2011) but it is right that it was missing in this manuscript. We therefore added the following details about its formulation and amplitude in the new version:

"One of the major challenges is to ensure the model stability in the highly energetic western boundary current and the associated anticyclonic gyre called the Great Whirl, without using excessive momentum dissipation that would damp small-scale processes elsewhere. Momentum, temperature and salinity are therefore advected using a third-order, diffusive, upstream-biased scheme (Shchepetkin and McWilliams, 2005; Madec, 2008). The intrinsic horizontal diffusivity of this scheme is proportional to the current velocity u (1/12 $\Delta x^3|u|$, with Δx the horizontal resolution in m). The resulting dissipation is of the order of 6.10^{10} $m4.s^{-1}$ in the Great Whirl (where currents reach 1 $m.s^{-1}$) and 2 orders of magnitude lower in the central Arabian Sea (where currents are of the order of 1$cm.s^{-1}$). This scheme does not require any additional dissipation and diffusivity to ensure numerical stability."

5) page 5515: Laure, equations should be viewed as part of sentences, and so punctuated accordingly. So, in the above equations, I added a period at the end of them.

Thank you.

6) page 5515: I thought that f(O2) was long and complicated enough to be displayed in its own line. I am still not sure, though, about whether it is written correctly. For example, I am not sure what "0,6□□O2" means. I guess it means that max(0,6□□O2) = 0 if O2 > 6 and is zero otherwise. Right? Please check.

Indeed, the formulation of f(O2) was unclear (but right) and the text explaining it was wrong. We arranged the formula and changed the text accordingly:

"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:

(Equation).

When O2>6 micromol/L (f(O2) = 1), remineralisation is aerobic and only consumes oxygen. However, when O2<6 micromol/L (f(O2)<1), part of the organic matter remineralisation consumes nitrate instead of oxygen (denitrification)."

7) page 5515: From our own work on this topic, I know that the detrital sinking (ws) and remineralization (e) rates are very important for setting the depth scale of the OMZ, dz = ws/e. So, at this point in the text, I wanted (expected) you to tell me what those rates are. Should you do that? Maybe not, as most of your readers are likely not aware of the OMZ sensitivity to dz.

The relationship between the OMZ depth scale and the sinking speed to remineralisation ratio is not straightforward in our model. Indeed, the model distinguishes between small slowly sinking particles (D) and rapidly sinking large particle (DD) for which sinking speed increases with depth:

ws(D)= 3 m/d

ws(DD) = wmin + (wmax – wmin)* max(0, (z-MLD)/ 2000 m)),

with wmin=50 m/d and wmax=200.

Oxygen concentration depends on DOC remineralisation rate (0.3 d-1) and therefore only indirectly on D and DD degradation rate (0.025 d-1). In addition, aggregation and disaggregation processes between the DOC and the 2 detrital compartments are parameterised to take into account differential settling and turbulence coagulation mechanisms (Aumont et al. 2003) and reproduce observed increase in the relative abundance of large particles with depth and the presence of small particles at depth.

However, if we compute dz(D) and dz(DD) using the range covered by ws(DD) (i.e. 50-200 m/d) we obtain dz(D) = 3/0.025=120 m and dz(DD) = 2000-8000 m.

In order to clarify the model formulation for detritus we added the following sentences:

“ DOC concentration and therefore oxygen concentration are highly dependent on the detrital particles distribution in the water column. To account for the range of particle sinking velocities observed in the ocean, the model distinguishes between small slowly sinking particles (sinking rate of 3 m.d-1) and large particles with a depth increasing sinking rate (50-200 m.d-1). Aggregation and disaggregation processes, which are necessary for reproducing the observed increase in the relative abundance of large particles with depth and the presence of small particles at depth is represented using a turbulence parameter and the concentrations of DOC and small and large detritus (see Appendix of Resplandy et al. 2011 for further details).”

I am not sure that the details on dz should appear in the model description as we do not discuss the sensitivity to this parameter. However it could easily be done if judged necessary and helpful.

8) page 5515: How does the model differentiate between new and regenerated production? Should you tell your readers here? Or is the biomodeling community generally aware of this point?

We added some brief precision on this point: “calculating the uptake of nitrate and ammonium by phytoplankton separately”.

9) page 5516: I replaced “were damped” with “are relaxed to WOA05 values” in the above. Okay?

Okay, Thank you.

10) page 5516: Does the Kone et al. (2009) simulation develop an eastward shift? If so, to what degree was the shift initiated simply by the initial conditions of that run, which are oxygen from WOA05.

The Kone et al simulation presents the eastward shift which is indeed part of the initial state (WOA). However, this global simulation has been spun up for almost 250 years, which gives us confidence that the solution is at equilibrium even at 1500 m depth. In addition, the eastward shift in this simulation is overestimated as the western coast of the Arabian Sea presents oxygen levels higher than the WOA05. This bias confirms that the eastward shift is maintained in the model and is not only reflecting initial conditions.

11) page 5516, description of mixing: What about vertical mixing in the biological model? This process should be very important in the dynamics of the deeper OMZ. Is it the same as for the physical model?

It is indeed the same as for the physical model and it is now mentioned in the manuscript.

12) pages 5516—17: My key concerns about this paper stems from the length of integration of the two experiments: main run for only 10 years, and perturbation for only 33 years. Is a 10- year integration long enough for the model to adjust at all away from its initial conditions? Certainly not at depth. I expect the only changes that can occur are in the upper ocean, in the precise nature of the oxycline variability and perhaps somewhat below that depth. Regarding the perturbation run, is it problematic that oxygen only decreases at depth by 20 µmolO2/kg. There is no indication in the oxygen field itself that an eastward shift will ever develop. It may be true that "the experiment is useful for illustrating the mechanisms controlling the OMZ formation," but it can't really say anything about the biodynamics of the eastward shift.

I think you need to defend your experimental design in more detail here. I note that you state in a comment that it would take 500 years for the perturbation run to reach equilibrium (longer than in our model). That is likely true for your main run as well. So, how does this lack of equilibrium affect your conclusions? Just what can you actually conclude with certainty?

Concerning the perturbation experiment, we agree that it was not easy to understand what the it represents. We therefore decided to focus the paper on the 10-year realistic simulation and took out the perturbation experiment.

Concerning the main (and only remaining) experiement, the spin up is actually longer than 10 years but the information was unclear and the reader had to refer to three other publications. We therefore added information in the model description (spin up details) but also in the result section. Figure 1 (below) has been included in the manuscript now shows the model drift .

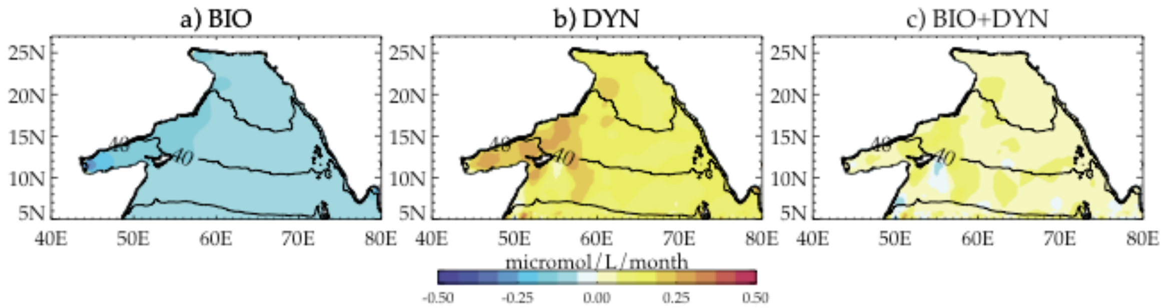

Fig 1: Simulated trends of oxygen averaged between 200 and 1500 m: a) biological term , b) dynamical term and c) the residual term. Units are in µmol.L−1.month−1. Contours indicate the oxygen concentration in µmol.L−1 averaged between 200 and 1500 m.

The following additional details on the spin up were added in the model description:

"The regional configuration used in this manuscript has indeed been integrated from 1992 to 2003. However it was initialised with a climatology constructed from a global simulation at 1/2° horizontal resolution (Koné et al., 2009). This global model has itself been spun up for 243 years (200 cyclic years and 43 years with interannual forcing from 1958 to 2001). The global simulation at lower resolution enables to spin up the physic and biogeochemistry, which would be extremely expensive if performed with the high resolution regional configuration. The discontinuity created when the regional simulation is initialised with the global simulation is much smaller than if the WOA was used. Indeed, a sensitivity experiment initialised with the WOA data was performed and showed a tremendous drift compared to the present simulation. However, we agree that the differences in the physical model (1/2° vs. 1/12°) and the biogeochemical model (iron and phosphorus compartment were taken out for the regional 1/12° simulation) explains why the model has not reached full equilibrium. The model drift in oxygen content is < 0.2 µmol.L/month between 1995 and 2003. This residue represents a drift

of the order of 0.5% per year and is smaller than the seasonal amplitude of oxygen content in the same depth range that reaches 3-5 µmol/L/yr."

13) page 5517: What do you mean by "surface layer"? Is that the top-most level of the OGCM? Or is it a surface mixed layer? Please explain.

The text has been modified as follows:

"Jflux [...] impacts the top-most level of the model and consequently slightly modifies the concentration in the mixed layer, which is located above the OMZ."

14) page 5518: In the above you refer to the "western coast." Do you mean the west coast of India or the west coast of the Arabian Sea. Please clarify. This potential confusion occurs elsewhere as well.

It is the west coast of the Arabian Sea. The text has been changed to avoid confusion here and in many other sentences.

15) page 5519: I was confused by your used of the term "core" here and elsewhere. For me, the core of the ASOMZ lies at depth, in a depth range centered about the depth where oxygen attains its minimum value. I am not sure how you define that term, but it often seems like you use the term to refer to a region just beneath the oxycline. I think you need to define up-front the various depth ranges in the OMZ that you refer to. Perhaps useful labels are "oxycline," "upper OMZ," and "lower OMZ."

See below.

16) page 5519, lines 12—14: Coastal upwelling extends only to about 150—200 m. So, this process cannot impact the "core" of the OMZ. (But that depends on just what depth range you mean by "core.") Fix the text.

See below.

17) page 5519, lines 17—18: What feature are you referring to here? The slight shift of the isoline? Please tell your readers.

See below.

18) page 5519, lines 24—26: What feature are you referring to here? The observed and modelled distributions have a very di§erent structure. So, it is hard to know in what way the model is more intense.

Comments 15 to 18:

We agree that these two paragraphs lacked clarity and were therefore rewritten. The "Core", "Upper" and "Lower" OMZ are now defined clearly at the beginning. The dynamical processes such as coastal upwelling etc... that were a bit speculative in this paragraph are now described later in the manuscript once the pertinent diagnostics are shown (figures of currents, oxygen transport...). Finally, we hope that the features we are referring to are now clearly identified. The text is now as follows:

" Here we examine how the OMZ and its seasonality in the model compare to the World Ocean Atlas 2009 (WOA09) seasonal climatology. In this study, we distinguish three depth ranges of the OMZ: the "core" delimited by the 20 micromol/L isoline (Paulmier et al., 2009), the "upper OMZ" between the 100 micromol/L and 20 micromol/L isolines that extends from the oxycline to the core and the "lower OMZ" between the 20 micromol/L and 100 micromol/L isolines located at the base of the OMZ. [...].

Despite the relatively low number of observations, seasonal variations of oxygen concentrations appear in the WOA09 climatology (Fig.). The oxycline is uplifted by 50-100 m along the western, eastern and northern coasts where upwelling occurs during the SWM (see

100 micromol/L isoline on Fig. a,c). The depth of the oxycline is also modulated seasonally in the central Arabian Sea. The 100 micromol/L isoline domes in the southeast (south of 15°N and east of 65°E, Fig. a and c) and is deepened in the northwest (north of 20°N and west of 60°E, Fig. a and c) during the NEM, while the opposite is observed during the SWM. At depth, the seasonality is very weak: the 100 micromol/L isoline located around 2000 m is slightly uplifted during the SWM in comparison to its depth during the NEM (Fig. a,c). Note however that few observations are available at depth greater than 1000 m, which may not be sufficient to resolve such a weak seasonality. The model captures the major seasonal features, including the variations of the oxycline depth and the intensification of the eastward shift during the SWM (Fig. b,d). However, the two model biases described previously also affect the representation of the seasonal variability: the weaker oxygen vertical gradient in the oxycline results in weaker variation of the oxycline depth (Fig. b,d); and the increase in oxygen concentration along the west coast of the Arabian Sea during the SWM extends further vertically and laterally offshore (Fig. b). "

19) page 5520: In the above, I replaced "dynamical transport" with "oxygen advection" and "advection." That seemed clearer to me. Okay?

That seems okay to us.

20) page 5521, line 3: In the first sentence of the paragraph, you refer to the "core" of the OMZ. Not sure what you mean here, but it appears to refer to the oxygen levels in the vicinity of the oxycline. To me, that is not the core.

This has been changed following your advice on definition (lower, upper OMZ and oxycline, see comment 15-18).

21) page 5521, lines 14—16: I thought the Red Sea was closed. So, how can advection from the Red Sea impact anything. Also how can southeastward advection from the Red Sea impact the CAS region, which lies north of the Gulf of Aden?

The Red Sea is indeed closed by a boundary located at the Bab el Mandeb straight. However, like all other closed boundaries the solution is relaxed to observations over 12 grid points (with a damping coefficient decreasing from the closed boundary to the interior of the model). This allows water masses with characteristics of the Red Sea, the Persian Gulf and the Indonesian Throughflow to enter the model domain. However, we agree that the process we are talking about here has nothing to do with the Red Sea and therefore modified the text accordingly.

In either case, from what you have presented in this paper how have you demonstrated that horizontal advection "ventilates" (if that is actually the correct word, see above comment) the offshore regions?

Figure 6 1d showed that horizontal advection increases the oxygen concentration in the CAS so we concluded that CAS was indeed 'ventilated' (if this is the correct word) by horizontal advection. The origin of water masses involved was described but not shown in the previous version of the paper. The vertical velocity and the main component of horizontal velocity are now shown along with oxygen budget trends. Figure 6 was therefore separated into 3 figures presenting the three regions (CAS, OMA or IND). This allows a description of the origin of the different water masses. See below for this figure in region IND:

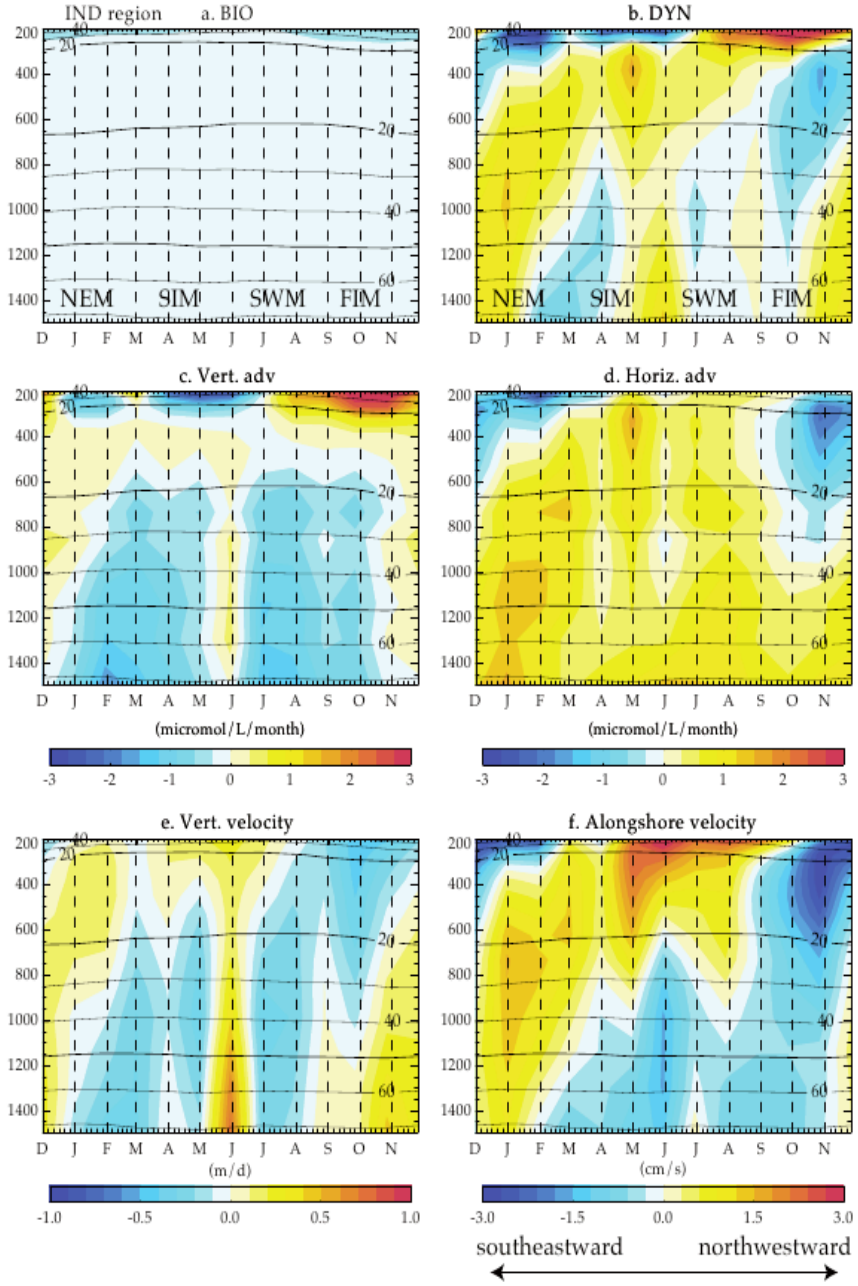

FIG 2: Seasonality in IND region between 200 and 1500 m in the model. a-d) Components of the oxygen budget (μmol.L−1.month−1) with the biological trend (a), the dynamical transport (b) and the contributions of vertical advection (c) and horizontal advection (d) to the dynamical transport; e) Vertical velocity (m.d−1); f) Alongshore component of horizontal velocity (cm.s−1), where values >0 (<0) indicate northwestward (southeastward) velocities. Contours indicate the oxygen concentration in the region (μmol.L−1).

22) page 5521, Figure 6: What you mean by the “2” in Figure 6(2) is not defined. That nomenclature needs to be defined somewhere. Likewise for 1 and 3.

Indeed, labels 1 to 3 were missing. The Figure has been corrected so labels are defined.

23) page 5521, line 21: Again “core” seems actually to refer to the very top of the OMZ, in the depth range of the oxycline itself.

As previously, this has been changed following your advice.

24) page 5521, lines 28—29: Are there any westward currents that actually carry low oxygen values from offshore to the coast during the NEM? I don’t know of any? Again, just what level does “core”

refer to.

In the model (see Fig 1f in Resplandy et al., 2011) but also in drifters observations (see Fig 1a in Resplandy et al., 2011 or Fig 31b in Schott and McCreary, 2001 for example), the circulation is westward in the central Arabian Sea during the NEM.

The sentence has been modified so there is no confusion with the 'core':

"During the FIM and NEM periods, the circulation reverses and low oxygen waters originated from the central Arabian Sea are transported into OMA (Fig 6)."

25) page 5522, lines 1—3: Here and elsewhere, it is important to differentiate between horizontal and vertical advection. In this instance, the oxygen tendencies in Figure 6 are surely due to vertical advection below 400 m, and only the near-surface changes result from horizontal advection. By the way, the deep vertical advection cannot be due to coastal upwelling (which is shallow), but rather to Rossby-wave propagation.

Indeed the differentiation between vertical and horizontal contributions was not clear in the text although they are shown separately on Fig 6, 7 and 8 (one for each region). We modified the figures by adding horizontal velocities (see answer to comment 21) and the text to clarify this point:

" During the FIM and NEM periods, the circulation reverses (Fig. 1 h). Low oxygen waters originated from the north, where the OMZ is more intense, are horizontally advected into OMA (Fig. 7 f). It is interesting to note 325 that the contribution of horizontal advection to the oxygen budget in OMA extends down to 800 m throughout the year. However, the lateral advection of ventilated waters by the coastal current during the SIM and SWM periods is much larger than the advection of low oxygen waters from the northern Arabian Sea during the FIM and NEM periods (Fig. 7 d)."

26) page 5522, lines 13—17, coastal undercurrent: What feature are you referring to here. It is hard for me to see a clear indication of a Coastal Undercurrent.

Indeed, figure 6 3d showed the advection of oxygen and did not specifically show the currents.

The oxygen budget in the IND region is now showed on Fig 8. Alongshore currents were added in panel f . We provide here a figure showing the alongshore current from the surface to 1500 m in IND. It appears that the current described in the paper (below 200 m) is indeed the undercurrent, which is flowing in the opposite direction from the surface current.

For clarity reasons, only the subsurface is shown in the manuscript but the text explains the presence of a reversed surface and subsurface circulation.

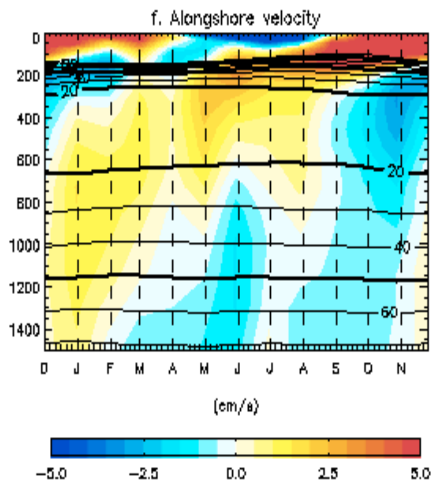

Fig 3: alongshore currents in IND region

27) page 5522, line 20: As in the previous comment, it is not clear what you are referring to here by surface confined and subsurface currents. I made a small change here, but can you clarify what you mean more.

This sentence was not clear and not very useful for the paper so it was removed from the text.

28) page 5522, Figure 6: the 1000 m marks on the y-axis in panel c extend into the panel b. Panel b

is mislabelled panel c.

This has been changed in the new version.

29) page 5523, lines 15—19; discussion of eddy terms: What have you presented in the paper that allows you to conclude that eddy-driven ventilation is mostly associated with the vertical advection of surface ventilated waters during the SIM and SWM and with horizontal advection during the FIM and NEM period? None of your figures or discussion seems to allow this conclusion. At the OMZ base is anything really advected into the OMZ itself? I doubt it. It is just the bottom of the OMZ shifting up and down slightly seasonally.

The results and discussion about eddy terms has been completely re-written and substantially extended. We now present and discuss the spatial distribution of the mean, eddy-driven, horizontal and vertical component of the oxygen transport with a new figure that includes the contribution of each term, but also the annual mean horizontal circulation and the mesoscale circulation:

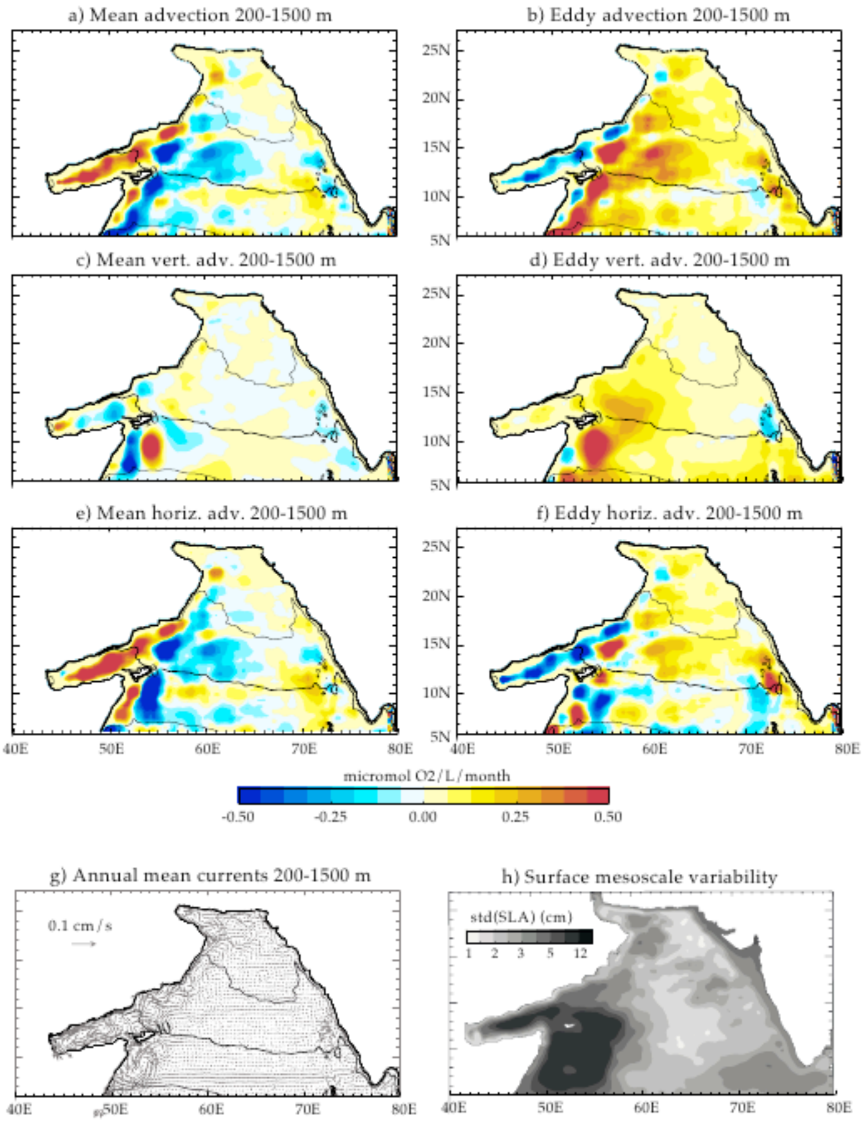

Fig 4: Simulated annual trends of oxygen between the 200 and 1500 m: a) mean transport, b) eddy-driven transport, c) mean vertical transport, d) eddy-driven vertical transport, e) mean horizontal transport and f) eddy- driven horizontal transport. Units are in μmol.L−1.month−1. Circulation: g) annual currents averaged between 200 and 1500m; and h) mesoscale circulation quantified as the standard deviation of the sea level anomaly (from Fig. 4 of Resplandy et al. (2011)). Contours delimits the 20, 40 and 60 μmol.L−1 oxygen concentration in average between 200 and 1500m.

30) pages 5523—24: Is your general statement about OMZs in fact true? Certainly, oxygen attains a minimum value in the water column in many regions other than just beneath areas of maximum

production. A key factor in the distribution of all OMZs is that they exist in a region of weak subsurface currents, which is therefore poorly ventilated. The extent of that poorly ventilated region determines the areal extend of the OMZ. Probably what you wrote is okay, but it may not be precise.

Following your advice, the text has been modified as follows:

"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 be one-dimensional processes: it is the consequence of 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."

31) page 5524: The reference here (and elsewhere?) to Figure 5, should be Figure 8.

Okay. Thank you.

32) page 5524, lines 5—6: I disagree with this sentence. In our paper on the ASOMZ, we discuss the dynamics of the eastward shift completely in terms of equilibrium solutions, by obtaining a large suite of test solutions. It is likely true, though, that the processes that account for the eastward shift cannot be deduced only from an analysis of (2). So, perhaps modify the first sentence to state your meaning more clearly.

As mentioned before the perturbation experiment has been removed. This sentence is not part of the text anymore.

33) page 5524, lines 11—14: It is hard to tell in Figure 8 just how close balance (@O2/@t)Bio =

□□(@O2/@t)Dyn is attained. Visually, the two fields do not seem to be close at all. It would be good to plot the di§erence of the two fields as well. You should argue that the di§erence map is "small" in some way with respect to the individual fields. Is that the case? Is there in fact a significant model drift? If so, the model is still not near equilibrium, which is a problem for drawing solid conclusions about the biodynamics of the seasonal cycle.

The figure inserted in our response to your comment 12 has now been included in the manuscript. It shows the difference between the 2 and gives an idea of the model drift. To be able to draw solid conclusion from a closed budget, we also decided to de-trend the budget by removing the residual from the dynamical trend. This is now explained in the paper.

34) page 5524, lines 15—16: It really is difficult for me to understand just what the perturbation experiment allows you to conclude. In particular, can the tendencies revealed by the experiment explain anything about the causes of the eastward shift? Perhaps, but it really is a stretch, since your final state is so far from equilibrium. You really need a more careful discussion of this point than you present in the current text. Just what can you, and can you not, conclude from this experiment?

See below

35) page 5525, Figure 10: I did not find Figure 10 to be useful at all. It just demonstrates that your solution isn't near equilibrium, which we already knew. I recommend that you delete it.

See below

36) page 5525, lines 5—7: The perturbation run does not produce an eastward shift at all. So, just how is the oxygen distribution in the solution spatially consistent with that in the observations?

See below

37) page 5526, lines 11—13: The claim in the last sentence is really just a hope. There is no solid

reason to expect it to be correct. You need to rephrase this unsubstantiated claim.

<u>Comments 34 to 37:</u> To address your concern about the OMZ formation, the perturbation experiment was removed so these paragraphs are no longer in the paper. The spatial distribution of the OMZ is now examined using the oxygen budget in the main run before focusing on the seasonality in the three key regions. We now present and discuss the spatial distribution of the mean, eddy-driven, horizontal and vertical component of the oxygen transport in the figure shown in comment 29.

38) page 5526, lines 22—25: Not sure if "upper part" is the correct terminology. The 15% change occurs where the oxycline is advected vertically, so occurs only at the top-most part of the OMZ. A more useful definition of "upper" OMZ might be the depth range from 200— 500 m, which lies below the oxycline. The "deep" OMZ is the part that extends to 800—1000 m.

See below

39) page 5527, lines 3—6: Your meaning is unclear. The "compensation" of oxygen va

riabil- ity between the upper (not near-surface) and deep OMZs must happen because of vertical advection (a consequence of the actual minimum of oxygen occurring near 500—600 m). This signal is likely due to a first-baroclinic Rossby wave. The near-surface signal is due to the swift monsoon currents and it must average out seasonally.

We agree that this was unclear. The text was re-written so the variability in the oxycline and the "compensation" are explained:

"In agreement with previous findings, the seasonality of the oxygen concentration in the OMZ is however weak (5% of the annual mean oxygen concentration) except in the oxycline, where it reaches of 15%. The seasonality in the model results from an imbalance between oxygen advection and biological consumption. At equilibrium the biological uptake is indeed compensated by the dynamical transport of oxygen, but this is not the case if seasons are considered separately."

40) page 5527, lines 11—29; page 5528, lines 1—2: There are a number of imprecise or un- substantiated statements in this paragraph.

We agree that the diagnostic on the spatial distribution were not strong enough, mostly because the perturbation experiment had not reached equilibrium. The result, discussion section and abstract on this point have been re-written using the main run to perform the diagnostics.

(line 14) How did you demonstrate that "low oxygen waters [in the central Arabian Sea] were sustained by their transport o§shore [from the Somali and Omani coasts0"? I can't find anything in the paper that shows this property. Delete or clarify. (lines 16—17).

This sentence has been removed.

Since there is no connection to the Red Sea, how can this conclusion be valid? (lines 18 and 19).

There is a forcing from the Red Sea that enters the model domain through a damping at the closed boundary. However, in light of our new diagnostics this is not the main mechanism at play so the sentence was removed.

Coastal downwelling can extend at most to 200—300 meters. I don't think it reaches deep enough to impact the "core" of the OMZ. A simi- lar problem exists for coastal upwelling during the SWM. There are deep signals in your model: They must indicate the presence of first-baroclinic-mode (low-order-mode) Rossby waves generated by o§shore Ekman pumping. (lines 26-29) The lower OMZ (the meaning of "lower OMZ" should be defined somewhere) cannot be influenced by coastal upwelling or downwelling, but it can be a§ected by o§shore Ekman pumping. (page 5529, lines 1—

2) Not sure about just what is the Coastal Undercurrent in your figure. Such a feature is ac- tually hard to define because of the o§shore propagation of Rossby waves, which occurs so effiently away from the west coast of Indian, particularly the southwest coast where the Rossby radius is large.

We agree that the main process is Ekman pumping and not the upwelling. Concerning the undercurrent, the figure inserted in comment 26 presents the alongshore current in the IND region. I think that it shows the surface current and the undercurrent in the opposite direction. Rossby waves are indeed emanating from the coast and propagating westward, however they can not explain the horizontal input of oxygen associated with the northwestward advection (see figure inserted in comment 21 panel d and f). We hope that the new figure and the text will help on that point.

41) page 5528, lines 17—25; lines 25—29: There is nothing in this paper to support either of these conclusions. Please clarify or delete them.

See below

42) page 5529, first paragraph: Nothing in this paper to support these conclusions either. Maybe that is okay since this is a discussion section. On the other hand, in the main text you concluded that eddies were not really important. So, it seems odd that so much discussion is devoted to that topic here. Should some of this text be moved to the subsection where you discuss eddies?

The contribution of mesoscale has been extended and is now discussed in more details (see figure of comment 29).

43) page 5529, line 25, — page 5530, line 2: There is nothing in the paper that supports the conclusions in this text. In particular, how is it shown that low oxygen values in the interior of the Arabian Sea are influenced by low oxygen from the western basin. Furthermore, if that is actually case, at which depth range does it occur. It is not likely to occur at great depth since currents are weak there.

As mentioned before, the diagnostic on the spatial distribution were not strong enough. The result, discussion section and abstract on this point have been re-written using the main run to perform the diagnostics.

44) page 5530, lines 14—27; discussion of McCreary et al. (2011): In the first sentence, I thought you concluded that mesoscale eddies had little e§ect on the OMZ in your solutions, which would seem to contradict the McCreary et al. (2011) results. I am also not sure that the last sentence is correct, as di§erent sinking or remineralization rates have a large impact on our OMZ. If, however, ws and e are changed by the same factor, such that their ratio z is unchanged, then the impact on the OMZ is weaker.

The discussion about the McCreary et al. (2011) was difficult to write given the relatively short version published in the SIBER letter. Now that the complete study is available, the discussion about the eddy contribution has been improved. Also, as we do not discuss the remineralisation rate in our model study, we took the sentences out of the text to focus on the eddy contribution.

"The major role of northward lateral advection and eddy-induced vertical advection on controlling the spatial distribution of the OMZ is in agreement with the study of McCreary et al. (submitted). In a suite of sensitivity experiments, they identified the horizontal transport of oxygenated waters from the south and the additional vertical mixing induced by eddies as essential to properly resolve the OMZ. In contrast, they found that horizontal mixing associated with eddies is not needed. This result is in apparent contradiction with our finding that eddy-driven horizontal advection is a key process in supplying oxygen to the western central Arabian Sea and the southwestern Indian coast. This difference probably arises from the spatial resolution and the subsequent representation of eddies in the two models. Whereas mesoscale eddies are explicitely resolved in our model, they are parameterised by a mixing

process and not an advective process in the model of McCreary et al. (submitted). Eddy mixing is however unable to represent the horizontal transport mediated by the 1000 km long mesoscale filaments emanating from the coast of Oman or by the eastward propagating mesoscale eddies formed along the coast of India.”

45) page 5531, section 6: Is this section needed? Didn’t you just state all of your conclusions in Section 5? I would combine and shorten these two sections. (Or maybe the journal asked you to write Section 6?)

We agree that this section is not absolutely necessary. It was written to summarize the main findings. It can be removed to shorten the discussion.