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Biogeosciences Discussions

Interactive comment on "Will open ocean oxygen stress intensify under climate change?" *by* A. Gnanadesikan et al.

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Response to Reviewers

We thank the reviewers for the time spent reviewing this manuscript and for their helpful suggestions for changes.

We with to begin by acknowledging an issue raised by both reviewers, namely that the title and abstract promised rather more than they delivered. Reviewer 1 states that *"The paper thus does not really answer the question posed in the title, unless one adds something like "in a particular run of a particular model". apart from the change in convective activity at a few grid points off Chile what, perhaps, could be a model artifact, the manuscript is relatively thin on new science."*

Francois Primeau adds *"The authors do a good job of diagnosing what is going on in the model. What is not so clear is the robustness of the results. The biggest changes in the buoyancy budget responsible for the changes in the ventilation are localized in a few grid points. To what extent are the results reproducible in other Earth System Models is not at all clear to me. Of course one has to start somewhere so it is important for climate modeling groups to report their results. I am therefore supportive of publishing the paper."*

Prof. Primeau has captured why we think this paper does have value, in that a careful term-by-term analysis has not been done within any other model. We believe that our key findings from the term balance are that 1. a large relative change in suboxia can be driven by a relatively localized change in convection 2. this change in ventilation is primarily communicated to the suboxic waters by parameterized lateral diffusion and 3. that the resulting increase in oxygen is actually opposed by the changes in remineralization. All three changes represent important insights into why our model produces a surprising result. In terms of robustness of the first result, the answer is trickier. On the one hand, this is a relatively small area involving only about 20 grid points. However, the phenomenon seen in this area (convection to 200m) is actually observed in the real world, though not in our control model, and so we think it's worth pointing out that such a small convective source has the potential to play a major role in determining suboxia over a large region.

We would agree, however, that similar analyses would need to be done across many models in order to get a sense for the robustness of our results. We would therefore agree that a change to the title: "Explaining why open-ocean oxygen stress does not intensify under climate change in an Earth System Model" would be in order.

The robustness of our results can, however, also be examined by looking to see whether other versions of ESM2.1 also show a similar change. (Adding freshwater fluxes as suggested has the disadvantage of changing local heat balances, SSTs, and thus atmospheric circulation). Additional simulations made using the A1B scenario where carbon dioxide was allowed to affect the radiative balance but the concentrations felt by ocean biogeochemical cycles and land biogeochemical cycles were held constant and runs where it was allowed to affect ocean/land biogeochemical cycles, but the held constant at preindustrial levels for radiative purposes. The resulting suite of 6 runs shows changes in the mixing (Fig 1a below) and net oxygen fluxes (Fig 1b below) off of Peru that are completely consistent with those presented here. Additionally, smoothed interannual changes in air-sea oxygen flux track the August mixed layer depths in this suite of simulations. The results appear within the first half of the 21st century, suggesting that this signal is not sensitive to fine details of the forcing. While our results may thus still be an artifact of model construction, the signal is at least robust across multiple models within the CM2 series. We will add language and a figure showing this in the revised version of the paper. Unfortunately, complete term balances were not done for this suite of runs and with the change in machines at GFDL reproducing them exactly is presently a challenge.

Specific Comments: Reviewer 1

The results are not presented in a very concise way. It is not clear how the quantitative statements about oxygen supply and consumption were obtained (p 7016, Table 1) as the volume over which budgets are computed is not clearly indicated. If the volume is bounded by some O2-isosurface, this volume will also change in time, making any interpretation of changes in integral fluxes difficult. It is not clear over which area the curves in Fig1c are integrated. If it is the global ocean, how representative is this for the oxygen minimum zones?

We agree that our statement of what we were doing here was not clear and led to the reviewer's pertinent question. For each row, we fixed a volume over which we integrated in the control run, and then examined how the budget within this volume changed within the climate change run. Thus there is no effect from the change in suboxic volume. The first two rows in Table 1 are a global horizontal integral, while the last two are just for the points that are suboxic in the control run. We will make this

clear in the revision both by adding language to the caption and by adding an additional two rows that look at the same budgets for hypoxic volume. The key points of Table 1 are that 1. the balance in a volume which is bounded by a constant oxygen contour is very different than the mean balance over some depth range. 2. In a realistic ESM parameterized lateral diffusion plays a big role in determining this balance.

Repeatedly, the dominant role of O2 supply by mesoscale eddies is stressed. However, the model cannot resolve these eddies, and the result may be very sensitive to the way eddies are parameterized. This potential sensitivity should be discussed in more detail. The text should mention that the simulated mesoscale O2 supply depends on the chosen sub grid-scale parameterization.

This is a fair point, and we will make the point both when we describe the model and in the conclusions.

p 7011, l 18: please specify how this correlation is computed. Pointwise on the model grid points? Is there some weighting applied that accounts for different volumes of different grid boxes?

The correlation is pointwise on the model grid points, weighted by box volume. This will be made clear in the revision.

p 7011, l 23 please explain quantitatively what is meant by "far too large". The overprediction does not seem to occur only in the Pacific, but according to Fig.1 also in the Atlantic and perhaps in the Indian Ocean.

This is a fair point. If one takes a cutoff of 20 μM, recent work by Bianchi (Ph.D. thesis, 2011) shows that the volume of waters with oxygen concentrations lower than this is 17-18 Mkm^3. Bianchi's dissertation shows that oxygen is biased high in suboxic waters in the World Ocean Atlas due to smoothing and so gets higher volumes of very low oxygen than in WOA05 (13 Mkm^3). Our model predicts 61 Mkm^3. However, more than of this volume is below 1200m, much of it in the Eastern Pacific. If we limit ourselves to the depths above 1200m, we get a value of 27 Mkm^3, still too big, but not as far off.

p. 7014, l.11 "residual age"? residual O2?

This should have said residual O2. Thanks to the reviewer.

p 7016, l18ff. Table 1. It is not clear what volumes the authors chose for their oxygen budgets. If they limit themselves to suboxic zones (l.18), the volume would change under global warming, making it difficult to compare O2 fluxes at different times. Also, rising oxygen levels (l.29) should not have much impact on budgets of the suboxic zones, because O2 cannot rise much (in absolute units) within a suboxic region.

See comment above. As we use fixed volumes to calculate budgets this objection doesn't hold- but we certainly recognize that the reviewers wouldn't have known this. Because the oxygen does rise within a fixed volume (in some cases by over 100 μM), the remineralization rates can change substantially.

p 7020, l 14. If salinification is responsible, OMZ waters should be more saline. Is this the case? If so, please show/specify this.

This is an excellent suggestion. In fact what we find is that the waters in the core of the region where oxygen increases become saltier by 0.6-0.9 PSU and warmer by 5-6C. This is shown below in Fig. 2.

p.7020, l.24 what is meant by models "would have trouble with suboxia"?

The statement should read- "would predict overly large suboxic water volumes."

Specific comments: Reviewer 2

"did not necessarily increase under global warming." should be changed to "does not necessarily increase under global worming." or "did not increase in their global warming simulation."

Agreed. Will change.

page 7012 lines 7-8: "a linear decrease in emissions ...resulting in a further increase in greenhouse forcing over this time period" is unclear.

We meant to say that our run has an increase in the greenhouse forcing relative to the simulation done by Schmittner et al. (2008). We will change the wording to make this clear.

page 7014 paragraph starting on line 22 and Fig 3c. For the water represented by the red "dots" in Fig 3c, there are both positive and negative changes in O2 and age and while it is certainly true that the largest changes are associated with increases in O2 and decreases in age, it appears that the many (a majority?) of the points have shifted to lower O2 and larger ages. Is this true?

It depends on the exact metric. If the total oxygen is used, 74

It's hard to tell for sure because of the overlying blue points. Also the contour plot in Fig 3b shows decreases of more than 100 (units? please add the units in the caption or beside the colorbar) in the Arabian Sea and in the Indonesian through flow that don't appear in the red dots shown in Fig3c even though they lie between 40 S and 40 N. Why is that?

We will add units to the caption. Actually, they do appear on the plot, they form the bottom of the red point cloud, with oxygen drops of 80-120 μM and age changes of 10-30 years.

page 7015 line 10: Need to refer to Fig 3d when discussing the "solid black line"

Done

page 7015 line 24: change "so that biological cycling is..." to "so that the change in biological cycling is...".

Changed.

page 7016 line 6: the sentence does not parse, there is a word missing

The reviewer is correct. We added “control simulation” after “in the”.

page 7016 line 7-12: There is a sentence that starts "This trend arises because..." and another that starts "This increase arises because there is ..." Only the second of these sentences actually explains why there is a trend.

Actually the first sentence describes why there is a trend in the control run, while the second describes why this trend reverses under global warming. However, we recognize the lack of clarity and have reworded this passage.

page 7016 line 11-12: Is the increase in the advective supply due to a change in the strength (or direction) of the currents or due to a change in the O2 gradients? In other words, are the changes in the resolved circulation significant?

The difference is largely because of changes in velocity (patterns follow the velocity change), with a big difference in the net tropical upwelling.

page 7016 lines 24-25 and Table 1: How sensitive are the results to the size of the box? Isopycnal diffusive fluxes of O2 will depend on the O2 gradient so that the relative importance of advection relative to small-scale mixing might be sensitive to the position and size of the control volume. Some sensitivity tests would be appropriate.

There is some sensitivity to the cutoff level of oxygen, with lower and lower levels having larger and larger relative changes. In the revision we will show values for 88 μM, 10 μM and 5 μM in addition to 8.8 μM, showing how the balances change with level. In all cases, however, lateral diffusion is the largest term and increasingly dominates the balance as one moves to lower and lower oxygen levels.

page 7016: line 17 insert "of" to get "...higher values of nutrients..."

Done

page 7016 line 21: the word "accounts" should really be "balances" because eddies don’t consume oxygen.

Agreed. Changed.

page 7016 line 23: insert "shadow zone" after stagnant and before regions to make it more evident why Luyten et al. 1983 is referenced.

Done

page 7016 line 28: I believe the discussion should point to Eq. (1) instead of Eq. (2).

Correct. Changed.

page 7017: line 2-3: change to "results in excessive spreading of the oxygen minimum zones in the vertical."

Agreed. Changed.

page 7017 line 5: the word "jump" makes it sound like there is an abrupt transition (i.e. bifurcation) to a larger eddy-driven supply of oxygen. Is that really what happens?

Actually there’s more of a gradual increase in the local uptake of oxygen of Chile, as we’ll show in the revision. We will change this word accordingly.

page 7017 lines 9-18: should anything be said about the model resolution and the extent to which the authors think the features captured by the model are sufficiently resolved for the results to be robust?

This is a fair point, in that only about 20 grid points are involved in the change. We will point this out. However, it’s worth noting that in the real world convection to this depth is seen off Chile, so the models are doing something that’s realistic in this sense. One of the things our result highlights is how important such localized processes have the potential to be in the real world, as the amount of oxygen needed to supply the suboxic zones is relatively small, and convective regions tend to become limited in size by convergence of light surface waters.

page 7017 line 26-27: "associated with" should be changed to "that parameterizes the *mixing due to"*

Agreed. Changed.

page 7020 line 11: I'm not sure how to parse "climate change linked to circulation". Deleting "linked to circulation" or perhaps replacing "response to climate change" with "response to changes in circulation."

Changed to “response to climate change because of complex changes in circulation”

page 7021 line 14: change "that the much" to "that much", i.e. delete "the"

Done

Interactive comment on Biogeosciences Discuss., 8, 7007, 2011.

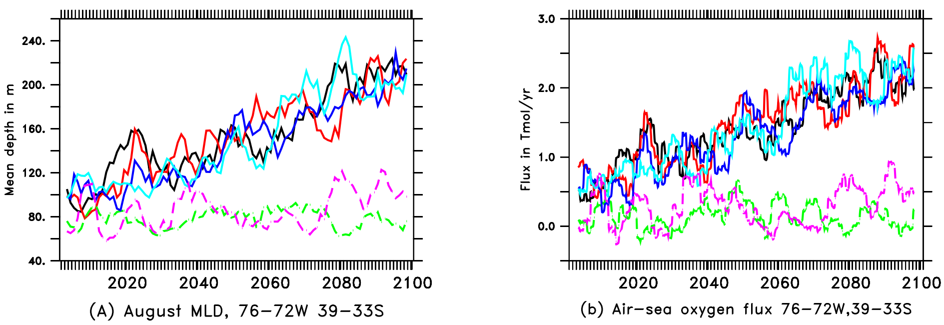

Fig. 1. (A) Change in winter MLD of Chile in runs with ESM2.1 under the A1B scenario. Solid lines show runs where CO$_2$ affects radiation, dashed lines where it does not. (B) Same as (A) but for oxygen flux.

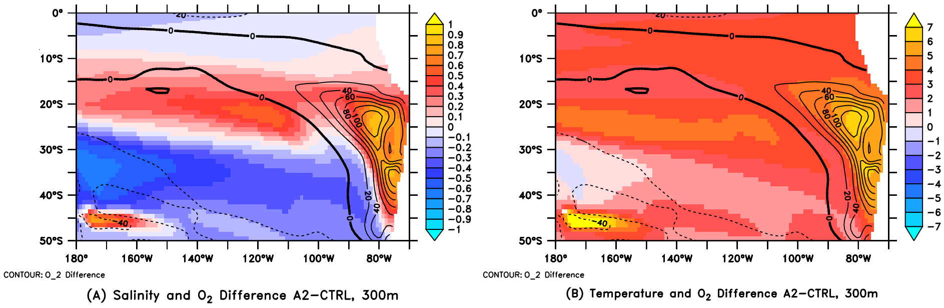

Fig. 2. (A) Change in salinity (A2-CTRL) at 300m in SE Pacific compared with change in oxygen. (B) Same as (A) but for temperature.