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Interactive comment on “Anticorrelated observed and modeled trends in dissolved oceanic oxygen over the last 50 years” by L. Stramma et al.

Anonymous Referee #2

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This paper addresses long term oxygen trends in oxygen minimum zones (OMZs) in several model experiments in order to test the hypothesis that enhanced diapycnal mixing and suppression of upwelling in coarse resolution models is the reason for an increase in oxygen in the OMZs over time in the models. This increase is in contrast to the observed expansion of OMZs that Stramma and others have reported on before and that is being re-examined in this paper by presenting time rates of O₂ change at 300 dbar from 1960 to 2010 for the world ocean based on the Hydrobase 2 data set. A comparison to 1920's Tropical and South Atlantic data from a Meteor expedition is also presented.

The paper is pretty straight forward, presenting observational estimates and a series of model sensitivity experiments with varying diapycnal diffusivities, C:N ratios, and

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forcing, but it is not incredibly deep. In some ways, it is an elaboration, with focus on the 300 dbar pressure surface, of the Duteil and Oschliess (2011) paper “Sensitivity of simulated extent and future evolution of marine hypoxia to mixing intensity” and also of the Keeling et al. (2010) review of “Ocean deoxygenation in a warming world”.

While this paper uses the same model and, as far as I can tell, the same model configuration as the Duteil and Oschliess (2011) paper, I am a bit puzzled by the apparent contradiction between the findings by Duteil and Oschliess (“marine suboxia shows a 21st century expansion only for mixing rates higher than 0.2 cm/s”; abstract) and the ones here (“this [model-data] discrepancy is not significantly reduced for substantially lower (or higher) levels of sub-grid scale mixing”; p. 4613). Hence, it would be good if the conclusions could be brought more into context with Duteil and Oschliess (2011).

One of the other conclusions that the model’s inability to resolve the equatorial currents and jets may be the reason why the data and models do not agree has been pointed at by Keeling et al. (2010) and also by the authors themselves (Stramma et al., 2010). In that regard, it would have been more satisfying if this paper could have also addressed model experiments regarding the oxygen supply via the equatorial currents. As is, all the experiments shown present a negative result since they do not help explain the observed oxygen decline in the OMZs. Inclusion of density changes at 300 dbar (in the observations and model) might help shed some light on how pathways change.

I would also like to comment on the title, specifically the term “anticorrelated”. I would replace it with something like “Mismatch [or Disagreement] between modeled and observed trends in tropical and subtropical oxygen concentrations over the last 50 years” since anti-correlation sounds like two processes that are both correct and just out-of-phase or in the opposite direction. Assuming that we can believe the data, the model simulations are the ones that are not correct in this case. However, the authors should include a map that shows the data coverage and address the issue of decadal variability in the subtropical and subpolar regions when trying to estimate linear O₂ trends earlier on in the observations section.

Overall, while the model results are not the most satisfactory, the paper still presents a coherent and self-contained piece of work suitable for publication (with some modifications) in the Biogeosciences special issue on “Low oxygen in marine environments from the Cretaceous to the present ocean: driving mechanisms, impact, recovery”. The figures are all of high quality and also suitable for publication.

Specific comments:

p. 4600, 1st paragraph: An additional figure showing the data coverage (perhaps color-coded by time) would be helpful.

p. 4601, l. 1-2: I cannot follow this sentence. Grammar?

p. 4601, l. 14-15: What’s an “interquartile range filter” and what’s the significance of first and third quartiles? I don’t think this method is standard knowledge in oceanography. Please give some reference and more specific information how this works.

p. 4601, l. 17-18: Some more references and detailed information on the techniques used would be helpful here too (e.g. for “scatter plot smoother”, “tri-cube distance weighting”, and the 1500km correlation scale chosen).

p. 4601-4603, section 2: What is the advantage of using a coupled model here given that the winds are prescribed anyway (p. 4603, l. 11)? Wouldn’t a hindcast ocean model (forced by NCEP reanalysis) work as well? That would allow the model resolution (vertical and lateral) to be much higher and to resolve equatorial currents better? Oxygen could even be run offline in that case, and model experiments altering vertical diffusivities for oxygen and other tracers only (i.e. w/o altering the circulation) could be performed. Please elaborate.

p. 4603, l. 24-25: I cannot follow this sentence. There seem to be some words missing. I presume the higher resolution achieved is supposed to be lateral?

p. 4603, l. 26: awkward wording: “employed large computational horizontal influence radius”

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p. 4603, l. 14-15: I think the statement “there are reports that the circulation in the subtropical gyres has intensified in recent years” is too general and superficial. There appears to be a lot of decadal variability in the ventilation of the subtropical (both northern and southern hemisphere) and subpolar gyres. Most the trends seen in Figure 1 in those regions is probably an artifact of the sampling times and locations. A figure showing the sampling times and locations (see above) would really help.

p. 4605, l. 9-20: How much does the density at 300 dbar actually change as O₂ changes? Is there any correlation? Whitney et al. (2007) did their analysis on isopycnal surfaces as well as many other ventilation studies. Hence, changes in isopycnal depths in the observations and in the model should be examined as well in order to compare properly.

p. 4606, l. 8: Interannual and decadal variability and their effects on the trends should be mentioned much earlier since they are significant part of it and likely introduce biases in the estimated trends, depending on sampling time (see above).

p. 4607, l. 2.-5: So, does this final sentence of section 3 indicate that the whole discussion of linear trends in the subtropical and subpolar gyres earlier in the section is flawed? Perhaps something other than a linear fit to the data should have been performed to account for decadal variability.

p. 4608, l. 21: What is the rationale that the low and high extremes of mixing, both give reduced (positive) O₂ trends in the tropics?

p. 4608, l. 21-24: How does the density at 300 dbar change for the different sensitivity runs (see above)? Is there any correlation with O₂ and isopycnal depth changes?

p. 4609, l. 10-11: How is the signal resulting from changes in transport pathways and processes calculated? Is it the residual of the total and the other processes? Generally, the paper could be improved by being more specific when discussing calculations/methods used and by explaining them better.

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p. 4610, l. 1-2: Transport pathways and processes are shown to have the largest contribution to modeled O₂ changes (though sign unfortunately is wrong compared to observations). Since climatological winds are used even in this coupled model configuration what exactly causes the transports to change? An increase in surface temperature (decrease in density) and in stratification under global warming conditions suggests that density changes at 300 dbar should be really looked at as well (see above).

p. 4611, l. 4-8: While the total correlation might not be any better, the model run with CORE-2 forcing (Figure 7b) does seem to show the largest the negative O₂ trends in the OMZs in the eastern tropical Pacific and Atlantic which I think is worth noting. Since elsewhere the observational trends are likely biased by decadal variability, estimating the correlation between observed fields and modeled CORE-1/CORE-2 experiments only in the OMZs would be useful.

p. 4612, l. 19-21: Again, the statement that subtropical gyres everywhere (?) have accelerated is too general. Roemmich et al. 2007 only investigated the South Pacific, and the spin-up may have already been reversed in recent years.

p. 4613, l. 10/table 1/figure 3: What are the uncertainties on the trend estimates?

p. 4613, l. 16-23: The results from this modeling study tend to be compared only to other coarse-resolution coupled models. A comparison to ocean-only simulations that focus on OMZs (e.g. by Deutsch et al. (2011) which is briefly mentioned earlier in the paper) is lacking here.

p. 4614, l. 4: “data briefly covers”? Please reword.

Technical comments:

p. 4599, l. 5: leave out “of” p. 4599, l. 19: missing word “of” after “difference” p. 4600, l. 27: “a” should be “an” p. 4605, l. 26: replace “than” with “as” p. 4606, l. 11, l. 14: Add “19” to year numbers, i.e. “1960’s”, “1980’s”, “1990’s” p. 4607, l. 1: “than” should be “as” p. 4611, l. 27-29: A reference to table 1 should be included when correlation is

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quoted.

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