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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.

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Reply: We thank reviewer 2 for the helpful comments. Our reply to the reviewer's comments is following after each reviewers comment.

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 diapyncal diffusivities, C:N ratios, and 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-ofphase 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.

Answer: We modified the title and used mismatch. The other comments are all answered below in the specific comments.

Specific comments: p. 4600, 1st paragraph: An additional figure showing the data coverage (perhaps colorcoded by time) would be helpful.

Answer: As requested, an additional figure showing the data coverage is now included as new figure 1.

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

Answer: We rewrote the end of that paragraph.

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.

Answer: A the first quartile is larger than 25% of the data, the 2nd quartile is the median (larger than 50% of the data) and the third quartile represents 75% of the data. The interquartile range is the value of the 3rd minus the 1st quartile, representing the center 50% of the data. An interquartile range filter does not require a normal (Gaussian) distribution and is thus a more objective way to quality control data in comparison to a standard deviation threshold (which does require a normal distribution). In case of a normal distribution, the data interval 3 times the interquartile range below the first and 3 times the interquartile range above the third quartile represent 99.997% of the data points. It has been used in high profile ocean related publications like: Global Sea Floor Topography from Satellite Altimetry and Ship Depth Soundings; Walter H. F.

Smith and David T. Sandwell; Science 26 September 1997: 277 (5334), 1956-1962. [doi:10.1126/science.277.5334.1956]. This information is now included in part in the text and the reference is also included.

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).

Answer: The LOESS tri-cube approach is weight = (1- (|distance/LengthScale|)ˆ3)ˆ3, it is commonly used for many LOESS problems, as introduced first by Cleveland(1979) when presenting robust weighted regression. This as well as 2 other references were added to better explain the methods used.

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) too 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.

Answer: The main difficulty in using a hindcast model is the strong sensitivity of the model results to the applied initial conditions. In order to obtain biogeochemical tracer distributions that are fully consistent with the model dynamics, spin-up periods of several thousand years are required. High-resolution hindcast models that start from observational estimates of “average” biogeochemical tracer distributions such as provided via the World Ocean Atlas, usually display significant trends during the first decades to centuries. Our own simulations with such models have shown that these trends can well dominate trends induced by the atmospheric forcing during the same period. Moreover, modeled trends are sensitive to the assumed initial conditions which, in turn, depend on the employed wind forcing prior to the 1960s, as shown in Figure 8. Because of oxygen-related non-linear biogeochemical feedbacks via denitrification and anammox on nutrient inventories, biological production and oxygen consumption, and judging from our own sensitivity experiments with high-resolution forced ocean models, it does not appear straightforward to simply "substract" results of a climatologically forced run from a run with interannual atmospheric forcing.

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?

Answer: Yes, higher lateral resolution. We rewrote the sentence.

p. 4603, l. 26: awkward wording: "employed large computational horizontal influence radius" Answer: We replaced the wording by "despite the large horizontal influence radius applied in the mapping routine".

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.

Answer: We rewrote the beginning of that paragraph and added a figure showing the spatial-temporal distribution of DO samples.

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 isopcynal depths in the observations and in the model should be examined as well in order to compare properly.

Answer: We have now included the density change at 300 dbar as estimated from the observations and simulated by the model. Results are shown in the new Figure 9 that

displays substantial model-data differences in the western tropical Pacific.

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).

Answer: The information on interannual and decadal variability was moved to the introduction.

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.

Answer: The information on the Meteor 1925-1927 oxygen trends was modified as differences are seen only at some areas, the data from the early period might not be as precise as later data and the method used was a little different than the method used for the 50-year trend computations. This is now mentioned in the text.

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?

Answer: At very low diapycnal mixing intensities, the warming-induced increase in stratification leads to a relatively large poleward movement of isopycnal outcrop areas, a corresponding increase in the distance waters have to travel from their outcrop region into the tropical thermocline, and thus decreased oxygen transport into the tropical thermocline. In the high-mixing case, the enhanced downward heat transport in the high-mixing case decreases solubility and hence thermocline DO levels. This is now explained in the text.

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?

Answer: All sensitivity runs using different mixing intensities show decreasing densities at 300 dbar almost everywhere in the global ocean. In the tropical Pacific, the simulated density change is almost completely explained by warming. We analyzed this point further by computing the density trend from the same bottle data used to in the observational estimate of 300 dbar oxygen trends. It turns out that observed density shows increasing trends particularly in the western part of the tropical Pacific. This is consistent with observed temperature decreases in this region (Harrison and Carson, JPO 2007). It is likely that part of the mismatch in modeled and observed DO trends is related to mismatches in simulated and observed density changes. This is now discussed in more detail in the revised paper. The observational estimate and the model-derived density trends at 300 dbar are now shown in a new Figure (Fig. 9).

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.

Answer: The reviewer is correct, the signal resulting from transport changes is computed as the residual of local total changes and local biotically driven changes. In the model, we have a full record of local changes, but it would require substantial additional work, such as employing specially designed diagnostic tracers, to diagnose non-local transport effects. The way the individual components were computed is now explained in more detail in the text and also in the caption of Fig.6.

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).

Answer: Overall, there is a decline in the strength of the overturning circulation un-

der global warming. Outcrop areas of isopycnals tend to move poleward, leading to changes in ventilation pathways and ventilation time scales. A comparison of simulated and observed density changes at 300 dbar is now included in the revised version (see answer to comment p. 4608 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.

Answer: In the first version we used only the difference between the decades 2001 to 2010 and 1958 to 1967. Now we used a linear regression instead, leading to a more consistent comparison with the observational trend estimate. The global pattern correlation between simulated and observed linear trends turns out to be even slightly positive for one of the two CORE runs, which is mentioned in the Table. In the revised text, we now point out the better agreement of the CORE-2 results with the observed trend in the eastern tropical basins. We are, however, reluctant to give a local correlation measure, as the number of degrees of freedom becomes small for small regions (the influence radius used for computing the trends from observations amounts to 1500 km). Our sensitivity runs with interannually varying wind forcing, which differ only in the initial conditions in year 1958, show that even the sign of regional DO trends over the period 1960-2010 is sensitive to relatively small variations assumed wind forcing prior to the analysis period. This is now discussed in more detail in the revised manuscript.

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.

Answer: The text was modified to include also the Deutsch et al. 2005 results from the

North Pacific and the Roemmich et al. results are mentioned now only for the South Pacific.

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

Answer: In the original version of the manuscript, all model "trends" were estimated by simply dividing the difference of the simulated DO fields for year 2010 minus year 1960 by 50 years. Because the model is forced by anthropogenic CO2 emissions, with climatological seasonally cycling winds, there is virtually no interannual variability in the model, making this pragmatic approach pretty accurate. In the revised version we nevertheless compute the linear trend at each grid point via a linear regression against the annual means from 1960 to 2010. Also computed is the estimated error of the trend, the spatial mean of which is now given in Table 1. The same procedure is used to compute the trend of the CORE wind forcing runs, which employ interannually varying wind forcing and thus display some interannual variability. Hence, uncertainties in the estimated trend are more than an order of magnitude higher in the CORE runs (see Table 1 of the revised paper).

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.

Answer: The 1 degree (0.5 degree latitude in the tropics) model used by Deutsch et al. was spun up for "only" 600 years, which means that much of the ocean interior is still considerably influenced by the initial conditions. Still, global patterns of thermocline oxygen levels (supplementary Figure S1 of Deutsch et al.) appear similar to those of other global models. As our intent is to show a first global view of observed DO trends in comparison with current model simulations, we therefore concentrated most of our discussion on models that are in equilibrium with the applied forcing. Our sensitivity experiments using different wind forcing further illustrate the sensitivity of the results to

the wind forcing assumed for the pre-observational period.

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

Answer: The sentence was reworded.

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 quoted.

Answer: The mentioned writing comments were modified as proposed, except for a reference to table 1 in the text p4611 27-29, as this paragraph was completely removed.

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