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Biogeosciences Discussions

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

Anonymous Referee #1

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general:

The paper addresses a very interesting scientific question: Although all models and current understanding seem to agree that total marine oxygen content will decline under global warming, some models show that suboxic areas may shrink. A recent paper by Duteil and Oschlies has shown this for one particular model, but is not yet completely clear what causes this spatial pattern of oxygen change. The authors of the current study find a similar decline of suboxic volume in their model. They go beyond the Duteil and Oschlies study by looking in more detail at the oxygen supply and consumption and find that, under global warming, supply to the oxygen minimum zones increases faster than consumption.

Unfortunately, the manuscript falls short of the expectations it raises in title and ab-

stract: The title of the manuscript suggests a general analysis of possible changes in marine oxygen patterns. The abstract clarifies that the authors investigate results from a single run of a single model. This model reveals some peculiar feature at a few grid boxes off Chile, supposedly triggered by changes in simulated precipitation patterns, which are arguably among the least robust projections of current climate models. 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". part 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.

As far as I know, the model used by Duteil and Oschlies does not include any changes in the wind field and therefore must generate similar results for a different reason. The claim that changes in convective activity are responsible for simulated changes in O2 is not very convincing and almost certainly does not apply to other models. To add substance to this central statement of the paper, a sensitivity experiment with constant convective activity (e.g. induced by some local addition of fresh water off Chile) should be performed. I recommend that a major revision that considers all points above and below should be done. This will help to arrive at a more appropriate and robust answer to the question posed in the paper's title.

specific comments:

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?

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.

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?

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

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

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.

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

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

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