

Interactive comment on “Multiple stressors of ocean ecosystems in the 21st century: projections with CMIP5 models” by L. Bopp et al.

Anonymous Referee #1

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Overall Evaluation:

This is a well written article addressing the uncertainty in model prediction of four potential stressors of marine ecosystems in a future changed world: warming, acidification, alterations to primary production and de-oxygenation. While the 10 CMIP5 models agree (mostly) on the warming and pH changes, they have much less agreement on the regional changes in primary production and de-oxygenation. In fact there is a surprisingly small number of regions that even agree on the sign of the change in primary production (PP). To me, this is the main take-home message of the paper: current models are extremely uncertain in how primary production and oxygen concentrations will change and I really like Fig 5 for this. The water mass analysis and the robustness analysis are very nice.

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Individual Science Questions and issues:

1) As stated above, my main take home was the level of uncertainty in models predictions of PP and de-oxygenation: Could this be emphasised more, and potentially even used as a cautionary note to the community to be careful on relying on any single model result to suggest future changes?

2) I wonder whether the "Multiple stressors" aspect of the paper is the best aspect to emphasis. Though the authors do look at 4 main stressors, the story here is a bit muddled: part of the introduction (e.g. pg 3631, lines 20-line 4 next page) and several place in rest of text (e.g. 3647, lin3 5-6), the authors mention how the multiple stressors interact with one another (e.g. impact of acidification on deoxygenisation), but (to my knowledge) none of the models parameterise these interactions/feedbacks. The models only capture each stressor separately. I think this needs to be more clearly noted. It could be that model results will be quite different with some multiplying effect of changes, and it would be good to clear on this. Also figure 13 showing where multiple stressors might coincide, but only from the model mean. This needs to be very clearly stated, and in fact where models don't agree, should be masked in some way.

3) This article is a nice intercomparison between models. And the authors provide a table (Table 1) to show some of the differences in the models. Since this is an intercomparison, much of the results depend on some of the parameterizations of the ecosystems/biogeochemistry. Though I do think more in depth understanding of the reasons for the differences would be nice, it is maybe beyond the scope of the paper. However, for the readers to think more about possible reasons, I think it important that the authors include an expanded (or new table) to give more details of the differences in the ecosystem/biogeochemical aspects of the model. For instance - how does each model treat grazing? Never mentioned in the paper, but differences in top-down controls could cause some of the differences seen in NPP and export. What nutrients limit productivity in each model (i.e. do some include different set of limiting nutrients)? A potentially large reason for the differences in O₂ changes might come from the param-

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eterization of sinking of organic matter and remineralization: it would be good to know how the different models treat these. The authors state that differences in treatment of temperature dependence of growth and remineralization are likely responsible for the difference seen in NPP changes: it would be good to know how different the model parameters are in this regard.

Others Specific Comments:

pg 3636, line 25: Why not leave out coastal zones in Taylor diagram calculations? Also note that the satellite derived NPP has large error estimates on it.

pg 3642, lines 15-30: Worth pointing out that in some regions of the biggest changes in model-mean NPP (e.g. Equatorial Pacific), the models do not agree even in the sign – this is contrast to the O₂.

pg 3644, line 23: I think the use of the word "highly" here is misleading. Taucher+Oeschlies looked at either a temperature dependence or not – there was not several "levels" of temperature dependence studied as the word "highly" implies.

pg 3645, line 1-2: The authors state that "...it is likely that that they use very different parameter values..."; but since the authors know what these parameters are from their various models, they might want to be more definitive on this point. See point 3 above.

pg 3649, line 1: I am more stuck by how large the ranges are in the different models results than that the results are "...distinct and relatively robust across the range of models..". You might want to change this sentence.

pg 3649, lines 20-23: It would be nice to see how the overturning changes for each model, even plotted against pH and/or O₂ changes: i.e. does the difference in overturning really explain the differences in the model results?

Technical Corrections:

pg 3628, lines 14-17: It would be good to put the ranges (or standard deviations) of the

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percent changes to SST, pH, PP, and O₂. To me, the range uncertainty is a main point of this paper. pg 3635: line 17: "but" missing between "components" and "differ"? pg 3641, line 25: "Oceans" has a space in it. Figure 2: Symbols are very small. Might be worth either putting both figures on same scale, or note on the caption that they are different. Figure 11: Caption needs to state that all 4 RCP's are shown Figure 12: Caption needs to state that these are for RCP8.5 Figure 13: This shows where model mean changes are the biggest. And yet in some of the locations where the biggest mean is predicted the models do not even agree on sign of PP or O₂ change. I think this figure should be redone with the regions where models do not agree masked in some way. Otherwise this can be mis-interpreted.

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