

Interactive comment on “Hazards of decreasing marine oxygen: the near-term and millennial-scale benefits of meeting the Paris climate targets” by Gianna Battaglia and Fortunat Joos

Anonymous Referee #1

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The manuscript describes results of a number of simulations with an Earth system model of intermediate complexity with respect to changes in oceanic oxygen content and specific ecosystem stressors, such as the volume of low-oxygen waters and the value of a metabolic index. The authors present a number of interesting findings, for example that deoxygenation peaks about a thousand years after stabilization of radiative forcing and oxygen recovers thereafter. It is, however, difficult to identify a main message. The benefits of meeting the Paris targets is mentioned in the title, but the manuscript quickly leaves this storyline, with no mentioning of the Paris climate goals after the introduction. Also, there is little information provided on shorter than millennial timescales - i.e. the near-term goal mentioned in the title is not discussed in the

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manuscript.

The discussion of uncertainties is limited to parameter uncertainties. However, systematic shortcomings of the intermediate complexity model, such as fixed winds and ice sheets or the neglect of sediments and nitrogen cycle feedbacks. These shortcoming may be much larger than those discussed in the manuscript. This needs to be discussed.

Overall, there is substantial new and interesting science in the work presented, but in addition to the absence of a clear storyline, the presentation is very descriptive and does not go into sufficient depth to really explain the interesting findings. I don't think the manuscript is ready for publication in its present form. Instead, the manuscript requires a major reorganization, possibly a new title and clearly a well-defined storyline.

Individual comments:

p.2, l.5 what is the justification for calling this 'is now a key scientific task'? For what? Why should people be interested on timescales of several millennia?

p.2, l.11 & 17. Hypoxia is defined, then suboxia is used. What is the difference (if any)? Why are different terms used?

p.3 l.28 Does this mean that winds are unchanged during the 8000yr global warming simulations? What are the implications of this? Could this explain the systematic differences with respect to paleo inferences about oxygen changes under global warming? I think this requires a detailed discussion.

section 2.3 The model evaluation is presented in a manuscript under review and not available to the reviewer/reader right now. Impossible to judge. I suggest to include maps and profiles of oxygen distributions in this manuscript.

p.6, l.15. 'deeper' > longer? This suggests that low-oxygen waters are simulated mostly in the deep ocean, whereas in reality they are located at a depth of a few hundred meters, so that the real-ocean low oxygen volumes should be more sensitive

for shorter remineralization length scales. This requires some explanation.

p.6, l.20ff Why is the recovery level for export so similar, and that for oxygen so different among the models?

p.6, l. 24 & 26 Why does the metabolic index scale linearly with forcing (i.e. equilibrium temperature) when it changes non-linearly with temperature?

p.9 l.2 Why is this representative?

section 5. It would be good to learn more about the critical factors that determine model-model differences in simulated changes and recovery of circulation and oxygen. e.g. model resolution? treatment of wind forcing? different biogeochemical assumptions? temperature effects on remineralization?

p.14, l.5ff For which year are the changes given?

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