

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 #2

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The authors describe results from a modeling study projecting long-term future ocean oxygen evolution for different carbon emission scenarios. As such it is within the scope of ESD. It is one of a relatively few studies that go beyond the centennial time scale and that consider millennial and multi-millennial timescales. I'm not sure which scientific question(s) the paper addresses. If there is one, or several, it may be useful to make this clearer in the introduction. Its title indicates that investigations are centered around assessment of benefits from the Paris agreement.

I have mixed feelings about the manuscript. There are certainly novel aspects. For example, calculations of a metabolic index or diagnostics of relationships between oxy-

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gen related changes and global mean equilibrium temperature. These may be useful for other scientists or policy makers.

On the other hand, there are statements (in the abstract, introduction, and conclusions) that sound like novel achievements but that in fact are not new and have been documented before (e.g. the long timescale for deep ocean oxygen changes). Another irritation to me were the short discussions of paleo oxygen changes in relation to the future projections presented. The paleo oxygen changes are a complex issue by themselves and I did not find the cursory discussion provided here helpful. There is substantial evidence that the glacial-interglacial changes were influenced by iron fertilization or some other biogeochemical process that increased macro-nutrient utilization during glacial periods (e.g. Schmittner and Somes, 2016, *Paleoceanogr.*, doi: 10.1002/2015PA002905), something that is not considered in the future projection simulations discussed in this paper. This makes even a qualitative comparison difficult if not impossible. Moreover, large changes in ice sheets and sea level occurred during glacial-interglacial changes, which are not considered here either.

The paper is sparingly illustrated and includes many statements that are not supported by evidence or figures. E.g. the authors claim they have separated different contributions to the oxygen changes (production, consumption, solubility), but not a single figure is shown illustrating those.

Even though the authors acknowledge that many processes are not considered in their projections (page 4, lines 26-28) they do not discuss the possible impacts of those omissions on their results. E.g. the large increases in suboxic zones projected for high emission scenarios will lead to increased denitrification and a reduced fixed nitrogen inventory, which will affect productivity on long timescales (e.g. Schmittner et al., 2008). On long timescales, we would expect ice sheets to change considerably (at least for the high emission scenarios).

Another weakness of the manuscript is that in many instances model responses are

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simply described but not explained or understood. My notes include lots of “why?” annotations as listed below.

Specific comments:

Title: The “near-term” does not seem to be a focus of the manuscript. The term is not mentioned anywhere else in the text.

Abstract lines 6-7: “Deoxygenation . . . forcing.” This is not a new finding and has been shown before, e.g. in Schmittner et al. (2008). Page 4 line 9: “production, consumption, solubility” results of this decomposition are not shown in the remainder of the manuscript

Page 4 line 16-17: “We show that the oceanic oxygen equilibration timescale is considerably longer than its thermal equilibration timescale”. The long oxygen equilibration timescale has been shown before (e.g. Schmittner et al. 2008). Perhaps more of a discussion of previous long-term studies (the ones cited in the previous sentence) would be useful to better understand what is new and what is not.

Page 5 line 12: Battaglia and Joos (2017) is not available

Page 5 lines 16-17: please define the quoted variables precisely. What precisely is the AMOC index? How was it calculated? The same for the Indo-Pacific overturning and export production.

Page 5 lines 26-29: This is not new. It has been shown before in Schmittner et al. (2008).

Page 6 line 7: Why do the lower emission scenarios lead to increased oxygen?

Page 6 lines 9-10: Why do lower mixing coefficients lead to larger decreases in oxygen?

Page 7 lines 5,6: are the production and consumption tracer results shown somewhere?

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Page 7 lines 19-20: why do higher forcing levels lead to these MOC changes?

Page 9 line 4: Why are subsurface ages younger?

Page 9 line 9: increased stratification is not shown. Is it really increased at equilibrium or is this just a transient effect? If it is increased is this due to temperature or salinity? Fig. 3 indicates that at least in the Atlantic stratification is not increased due to temperature although export production there is decreased.

Page 9 line 14: Why does the temperature anomaly develop there?

Page 9 lines 18-20: Is this shown somewhere?

Page 10: Part of the figure caption is missing.

Page 12 lines 4-5: what are these numbers based on?

Page 12 line 7: “comparatively strong” compared to what?

Page 12 lines 27-29: The discussion here is too simplistic. In the paleodata the deep ocean’s oxygen increased while it decreased in the thermocline. I don’t see evidence provided that this is similar to the model data. It is not similar to Fig. 3a, rather the opposite, I would say. Page 12 lines 30-31: I don’t agree with the statement “Proxies of past ocean oxygenation and ventilation reveal similar structural changes and mechanisms.” Increased nutrient utilization e.g. from iron fertilization also most likely played a role in glacial-interglacial changes (e.g. Schmittner and Somes, 2016, *Paleoceanogr.*, doi:10.1002/2015PA002905).

Page 12 line 31: It is not clear if the overturning increased. Changes in overturning strength remain controversial (e.g. Kurahashi-Nakamura et al., 2016, *Paleoceanogr.* doi:10.1002/2016PA003001).

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