Reply to reviews for the manuscript

Hazards of decreasing marine oxygen: the near-term and millennial-scale benefits of meeting the Paris climate targets

submitted to Earth Syst. Dynam. Discuss.

by Gianna Battaglia, Fortunat Joos

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We thank the two anonymous reviewers for their reviews and constructive comments. We much appreciate the effort and time committed by the reviewers.

We appreciate their main concerns. As a result, we reorganized the presentation of our results and provide additional information and additional figures as requested. Also, we remove the discussion on paleo oxygen changes as requested. The introduction and conclusion are largely re-written to strengthen the main messages.

Please find below our response to the comments by the reviewers and suggested text additions to the manuscript. A new manuscript, and a manuscript version with track changes is attached.

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 radia- tive 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 millen- nial timescales - i.e. the near-term goal mentioned in the title is not discussed in the manuscript.

In this manuscript, we compare and contrast the usual assessment timescale of climate change at the end of the 21st century, here defined as near-term, to the multi-millennial equilibration timescale of ocean biogeochemistry. The original Figure 1 and the original Figure 4 contrasted these near-term changes to the peak changes as simulated by our model. Also, all timeseries plots visually highlight the near-term timescale (A.D. 2100) to the multi-millennial equilibration timescale (A.D. 10,000). We clarify this point in the introduction by adding the following paragraph. In addition, we add more explicit mentioning of the Paris Agreement on various places in the manuscript.

> Given the long residence time of anthropogenic CO_2 in the atmosphere, and long equilibration timescales of the ocean overturning circulation, anthropogenic climate change will grow and persist beyond the end of the 21st century, the typical near-term assessment timescale of climate change (*Clark et al.*, 2016). Only few studies have assessed ocean biogeochemistry and the oceanic oxygen content beyond this near-term timescale. Available studies employ a range of physical and biogeochemical complexity levels from box models to general circulation models (GCMs). Oxygen concentrations are simulated to decline beyond the 21st century on multi-centennial timescales (*Matear and Hirst*, 2003; *Hofmann and Schellnhuber*, 2009; *Mathesius et al.*, 2015). Siumlations covering two millennia show a recovery phase thereafter (*Schmittner et al.*, 2008; *Yamamoto et al.*, 2015). In most studies, simulated oxygen concentrations have not reached new steady state

conditions at the end of the simulation. Low order Earth system models and Earth System Models of Intermediate complexity integrated by up to 100,000 years have demonstrated the potential for long-term ocean oxygen depletion in response to carbon dioxide emissions and the long equilibration time scales of ocean biogeochemical variables in response to carbon emissions (*Shaffer et al.*, 2009; *Ridgwell and Schmidt*, 2010). Multi-millennial simulations are therefore required to assess the full amplitude of ocean biogeochemical changes and new steady state conditions due to anthropogenic climate change.

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 shortcom- ing may be much larger than those discussed in the manuscript. This needs to be discussed.

We add this discussion to section 5 Uncertainties in O_2 projections:

Major physical limitations of our simulations concern prescribed winds and ice-sheets. Future model studies may include sensitivity simulations with prescribed changes in the wind stress over the ocean (e.g. *Tschumi et al.*, 2008) and prescribed meltwater fluxes or apply earth system models with interactive atmospheric dynamics and ice sheets. Our study, as is the case for most climate change simulations, do not include melting of continental ice sheets, which would tend to further (transiently) reduce circulation (*Bakker et al.*, 2016) and increase the equilibrium climate sensitivity.

We neglect a number of biogeochemical feedback mechanisms that could alter biological productivity in the surface ocean and by that change remineralization fluxes in the water column. Any mechanisms that would increase remineralization would tend to decrease the oceanic oxygen, and mechanisms that decrease remineralization would increase the oceanic oxygen content. Future studies may address feedbacks from sediment interactions and imbalances from riverine input and burial (such as *Roth et al.*, 2014; *Niemeyer et al.*, 2017), temperature dependent remineralization, and variable stoichiometry. Further investigations may also address nitrogen cycle dynamics and assess the interplay of denitrification and N-fixation and of external atmospheric and terrestrial nitrogen sources. The resulting impact on the fixed nitrogen inventory in the ocean are currently unclear.

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 dont 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.

We follow the suggestions of the reviewers and change the presentation of the results. We now show and describe first Figure 2, the temporal evolution of critical variables, and Figure 3, spatial changes in ecosystem stressors at peak decline. These are followed by Figures 1 and 4. In addition, we provide additional details on mechanisms following reviewer #2. We add additional variables as timeseries plots and include further variables as section plots for process attribution and discuss now mechanisms of change in greater detail.

The new organization, and more explicit mentioning of the Paris climate goals should justify the choice of our title.

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?

The Parties to the UNFCCC are '*determined to protect the climate system for present and future generations*' and the UNFCCC mentions '*the threats of irreversible damage*' in its Article 3. The parties of the follow-up Paris Agreement recognize '*the need for an effective and progressive response to the urgent threat of climate change on the basis of the best available scientific knowledge'*, and the Agreement notes '*the importance of ensuring the integrity of all ecosystems, including Oceans'*. It remains thus an important task to further the scientific knowledge on the impacts of global warming on the ocean, considering potentially irreversible and long-term changes that may harm ocean ecosystems and threaten the well- being of future generations. To this end, we project the response in oceanic oxygen content and a number of additional ecosystem stressors including warming, export production and a metabolic index for a range of warming targets, including the 1.5 and 2 °C targets mentioned in the Paris Agreement. We consider within the Bern3D Earth System Model of Intermediate Complexity (EMIC) changes over this century as well as long-term changes over the next 8000, recognizing the multi-millennial equilibration time scales of marine biogeochemical cycles.

The specific wording has been removed from the introduction in the course of the largely re-written introduction. The underlying reasoning is still conveyed.

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

Hypoxia and suboxia refer to different O_2 concentration ranges. Hypoxia, defined as $O_2 < 50$ mmol m^{-3}, refers to conditions leading to O_2-stress for many macroorganisms. Suboxia refers to lower O_2 concentrations, here defined as < 5 mmol O_2 m^{-3}, leading to anaerobic metabolism.

Text clarified as:

> O_2 is vital for aerobic organisms in the ocean and typical thresholds leading to O_2-stress for many macroorganisms (hypoxia) are around 50 mmol O_2 m^{-3}.
>
> Suboxic (<5 mmol O_2 m^{-3}) or anaerobic conditions can also lead to production of poisonous H_2S within sediments

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 differ- ences with respect to paleo inferences about oxygen changes under global warming? I think this requires a detailed discussion.

We removed the paleo discussion and added text on potential implications of wind changes in section 5 as detailed further above.

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.

Apologies. The manuscript is now available from GBC:

http://dx.doi.org/10.1002/2017GB005671, DOI: 10.1002/2017GB005671

p.6, l.15. deeper T¿ 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.

We change 'deeper' to 'longer'.

We do not suggest that low oxygen waters are simulated in the deep ocean under modern conditions. The paragraph concerns the global warming experiments. The Bern3D simulates low O_2 waters in the thermocline of the modern ocean as observed (see Figure 3, Figure 7a, Table D1 of *Battaglia and Joos*, 2018). This is mentioned in section '2.3 Pre-Industrial characteristics'.

We expand the text on the volume of low O_2 waters in section 3:

> Oxygen-poor waters ($O_2 < 50$ mmol m^{-3}, Fig. 2h) are simulated to transiently increase across all scenarios. The response is characterized by high uncertainty as introduced by the sampled parameters. Under new equilibrium conditions, the volume of low O_2 waters is reduced for low and intermediate forcing and remains higher than pre-industrial in the high forcing case.
>
> Turning to uncertainties in our perturbed parameter ensemble, we find that variations in the vertical diffusion parameter ($k_{diff-dia}$) dominate the uncertainty in the globally-averaged evolution of ideal age, sea ice cover, temperature and O_2. The modeled uncertainty in the volume of low O_2 waters is dominated by different values of the α_{aerob} parameter. Whether a threshold in O_2 concentration is met depends on the pre-industrial tracer distribution. Longer remineralization length scales bring more remineralization to depth, leading to higher O_2 consumption.

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

We amend the paragraph on export changes with the following explanation in the manuscript. Please also refer to Figure 1 here, illustrating the export anomalies across three scenarios for new steady state conditions relative to preindustrial.

> Global export production is simulated to decline over the first few centuries, and reach higher values under new steady state conditions (Fig. 2g). The decline is stronger for higher forcing, while the recovery level of global export production is similar across the scenarios. Bern3D transiently simulates decreased export in the mid- and low latitudes (Fig. 4c, see also *Steinacher et al.* (2009); *Battaglia and Joos* (2018)) as a result of increased stratification (Fig. A2c,f,i) and reduced nutrient concentrations in the surface ocean (Fig. 4b). In the high latitudes, the model simulates increased export production, as a result of less temperature and light limitation as surface waters warm and sea ice retreats. This pattern of decreased export in mid- and low latitudes and increased export in high latitudes is similar across the scenarios. Export production in the low latitudes fully recovers for lower forcing and partially recovers for higher forcing. The lower recovery level in the low latitudes is compensated by higher increases in the high latitudes for high forcing. The magnitude of positive and negative changes increases with forcing, but the global anomalies remain comparable at the end of the simulation.

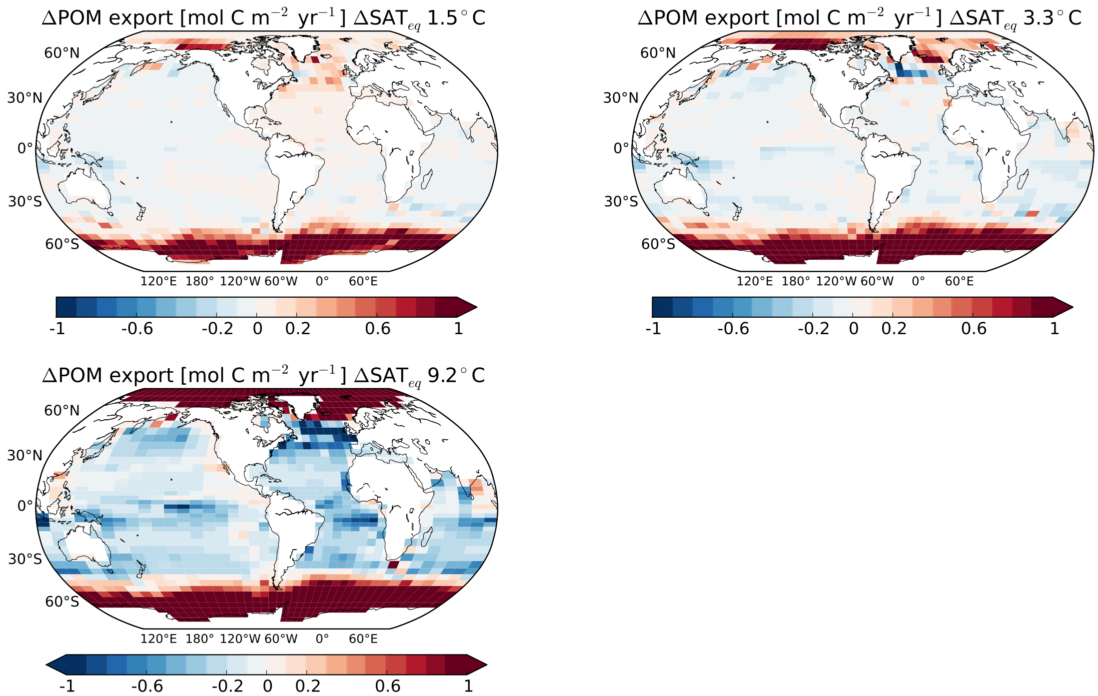

Figure 1: Export anomalies for new steady state conditions relative to preindustrial for the representative ensemble member and three different scenarios reaching 1.5, 3.3 and 9.2°C warming targets.

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?

The metabolic index changes as a result of changes in T and O_2 (see Figure 4 of the new manuscript). The metabolic index as a function of temperature can be approximated linearly from 0-15°C, and by another linear function in the temperature range from 15-35°C.

We add a sentence to section 2.1:

> One may note that the exponential curve varies approximately linearly for typical global warming associated temperature changes as $E_0/k_b(\approx 10{,}000$ K) is large.

p.9 l.2 Why is this representative?

It is a member with parameter values close to the standard/median parameter values. We add the following text to section 2.2 Ensemble and Scenarios:

> A normal distribution is used to sample α_{aerob} with a standard value of -0.83 and a standard deviation of -0.0625. α_{denit} is sampled uniformly between -0.1 and -0.01. And a lognormal distribution is used to sample $k_{diff-dia}$ (standard value=2.25E-5 m^2 s^{-1}, shape paramter=0.2, location parameter=0). We choose a single ensemble member with parameter values close to the standard values as representative ensemble member to illustrate spatial anomalies (α_{aerob}=-0.85, α_{denit}=-0.037, $k_{diff-dia}$=2.05E-05 m^2 s^{-1}).

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 assump- tions? temperature effects on remineralization?

We now provide additional information characterizing the models and better distinguish EMCIS from state-of-the art GCMs. We do not have the information to reliably assess the model-model differences and their influence on the simulated changes in O_2 and circulation.

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

Thanks. We clarify the text by:

> Figure 6a contrasts near-term (A.D. 2100) and peak changes (relative to 1870-1899) in measures of metabolically viable habitats in the upper ocean, hypoxia, and food availability as projected by Bern3D for a 1.5° warmer world. Export in low latitudes (30°S - 30°N) as an indicator of food availability is reduced by maximally 4% over the course of the simulation in this scenario. Median decreases in the metabolic index, representing viable habitat reductions of the upper ocean, amount to 11 % for a 1.5° warmer world.

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. Im 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.

The introduction is largely re-written. Please refer to the attached manuscript. In particular, we add:

> In this study, we assess the effectiveness of the Paris climate targets in reducing hazards of decreasing oceanic oxygen, ocean warming and marine export productivity as simulated by the Bern3D Earth system model of intermediate complexity. To this end, we prescribe in the model four different scenarios where anthropogenic GHG forcing is stabilized by 2300 AD either under stringent mitigation limiting equilibrium global surface air warming to 1.5 or 2°C above preindustrial or following business-as-usual 21th century emissions. Simulations are run to year AD 10,000 by which time the ocean has reached new steady state conditions. This allows us assess reversibility and the full amplitude of changes, which are larger than the near-term changes at the end of the 21st century. We summarize the outcomes developing global metrics which quantify avoided marine hazards per avoided global warming.

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

It is not our intention to make unjustified claims. We aimed to cite the relevant literature on long- term O_2 changes in the introduction and cited on p3, l.15: *Yamamoto et al.* (2015); *Matear and Hirst* (2003); *Schmittner et al.* (2008); *Mathesius et al.* (2015). We further discuss the findings of these and related studies in comparison with our results in section 5 (p. 12, line 10 to 24). Following the request of the reviewer, we provide a brief summary of the key finding of earlier long-term projections to allow the reader to better put our results in context of previous work. Please see the reply to reviewer #1 or the revised manuscript for the exact wording.

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 bio-geochemical process that increased macro-nutrient uti- lization during glacial periods (e.g. Schmittner and Somes, 2016, Paleoceanogr., doi: 10.1002/2015PA002905), something that is not considered in the future projection sim- ulations 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.

We acknowledge this point and remove the paleo discussion.

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 contri- butions to the oxygen changes (production, consumption, solubility), but not a single figure is shown illustrating those.

The original Figure 2e showed the explicit O_2 solubility term. The biological imprint of O_2 changes is given implicitly by the difference of the total O_2 tracer (Figure 2b) and the solubility tracer (Figure 2e).

We now provide new Figures illustrating the changes arising from the four O_2 tracers (total, solubility, utilization, production) as timeseries and as section plots (new Figure 2a,b,c,e, new Figure 3, new Figure A3).

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).

We now discuss these points in the discussion (5 Uncertainties in O_2 projections). It remains unclear whether the projected O_2 changes may lead to a reduced inventory of fixed nitrogen, as fixation of nitrogen may also change under global warming and elevated CO_2. Please see reply to reviewer #1 or the revised MS for the exact wording.

Another weakness of the manuscript is that in many instances model responses are simply described but not explained or understood. My notes include lots of why? annotations as listed below.

The primary focus of this MS submitted to the ESDD Special Issue on 'The Earth system at a global warming of 1.5°C and 2.0°C' is to document changes in measures of marine oxygen in relation to the Paris temperature targets. We now provide additional mechanistic explanations as requested. See answers to specific points 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.

We clarify that 'near-term' is used for 21st century changes as assessed in most studies on climate change in the introduction. Please see our answer to reviewer #1 on this point for the exact wording

Abstract lines 6-7: Deoxygenation...forcing. This is not a new finding and has been shown before, e.g. in Schmittner et al. (2008).

We are aware of the *Schmittner et al.* (2008) study; the study is cited 3 times in the submitted MS. We clarify in the revised MS that our projections cover a substantially longer time period than addressed in earlier simulations (8000 compared to 2000 years covered by *Schmittner et al.* (2008)).

We amended the sentence:

> Deoxygenation peaks about thousand years after stabilization of radiative forcing and new steady state conditions establish after AD 8000 in our model.

We now discuss the findings of *Schmittner et al.* (2008) and other studies in the introduction (see answer above).

Page 4 line 9: production, consumption, solubility results of this decomposition are not shown in the remainder of the manuscript

We added new figures (new Figure 2a,b,c,e, new Figure 3, new Figure A3) to show these results.

Page 4 line 16-17: We show that the oceanic oxygen equilibration timescale is consid- erably 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 dis- cussion 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.

We discuss the finding of the *Schmittner et al.* (2008) and other studies in the introduction (see answers above).

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

Apologies. The manuscript is now available from GBC:

http://dx.doi.org/10.1002/2017GB005671, DOI: 10.1002/2017GB005671

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.

Added text:

> The maximum of the Atlantic meridional overturning streamfunction below 400 m depth (AMOC) ranges from 16.5 to 19.7 Sv. The minimum of the Indo-Pacific meridional overturning streamfunction below 400 m depth (Indo-Pacific MOC) ranges between -13.6 to -15.6 Sv. Export of particulate organic matter at 75 m ranges from 9.0 to 11.4 Gt C yr^{-1}.

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

Now discussed in the introduction (see answers above)

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

The explanation on changes in Section 3 is improved. Please refer to the attached manuscript. In brief, a more vigorous circulation at the new compared to the PI steady state leads to an increase in ocean oxygen counteracted by a solubility/warming-driven reduction in oxygen.

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

Thanks for this question. The statement was in fact wrong, and the opposite is true. A full attribution of physical changes is beyond the scope of the manuscript. The statement is removed from the manuscript and instead, we add the following to section 3 Marine changes in temperature, circulation and biogeochemistry:

> Turning to uncertainties in our perturbed parameter ensemble, we find that variations in the vertical diffusion parameter ($k_{diff-dia}$) dominate the uncertainty in the globally-averaged evolution of ideal age, sea ice cover, temperature and O_2. The modeled uncertainty in the volume of low O_2 waters is dominated by different

values of the α_{aerob} parameter. Whether a threshold in O_2 concentration is met depends on the pre-industrial tracer distribution. Longer remineralization length scales bring more remineralization to depth, leading to higher O_2 consumption.

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

We added new figures (new Figure 2a,b,c,e, new Figure 3, new Figure A3).

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

We improved section 3 and show additional physical variables in additional figures in the main text and the appendix (sea-ice, streamfunction, temperature, salinity, density) for transparency and discuss these physical changes and their relationship. Please see section 3 in the revised manuscript.

Page 9 line 4: Why are subsurface ages younger?

Improved description in section 3:

> The warming perturbation causes the AMOC and Indo-Pacific MOC to decline transiently (Fig. 1e,f, Fig. A1). The larger the forcing and implied changes in stratification, the larger the peak decline in overturning (Fig. 1e,f). The decline is likely driven by upper ocean warming, leading to increasing surface-to-deep density gradients as further modulated by salinity changes (Fig. A2). The deep ocean water mass age increases in response to the slowed overturning (Fig. 2d, 3d). As retreating sea-ice increases wind stress over these newly exposed areas, younger water masses form in the upper ocean of the Southern Ocean (Fig. 3d).

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 salin- ity?

We add a new figure to the appendix (Figure A2c,f,i) illustrating anomalies in density for three different times (A.D. 2100, A.D. 3150, A.D. 10,000). The original paragraph referred to A.D. 3150. In new steady state conditions, the mid- and low latitudes are more strongly stratified. Decreases in density result from warming (Figure A2a,d,g), salinity tends to increase in the upper ocean (Figure A2b,e,h).

Fig. 3 indicates that at least in the Atlantic stratification is not increased due to temperature although export production there is decreased.

Density is non-linear in temperature, such that the anomalies have a distinct imprint on density depending on the background temperature distribution (Figure A2).

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

This feature could well result from internal redistribution of heat as the AMOC has slowed down transiently.

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

We add a new figure (Figure A3).

Page 10: Part of the figure caption is missing.

Apologies. Our version seems to be complete. We only add comments to subplots which are not self-explanatory.

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

Based on our simulations with Bern3D across different scenarios.

Page 12 line 7: comparatively strong compared to what?

We remove this statement. Deoxygenation in Bern3D is stronger compared to other available long-term simulations as outlined in paragraph 2 of section 5.

Page 12 lines 27-29: The discussion here is too simplistic. In the paleodata the deep oceans oxygen increased while it decreased in the thermocline. I dont see evidence provided that this is similar to the model data. It is not similar to Fig. 3a, rather the opposite, I would say.

The original Figure 3 showed the changes at peak O_2 decline. The discussion on Page 12 lines 27-29, however, focused on the respective equilibrium states. We add a new Figure A3 which shows the O_2 anomalies for new steady state conditions for a 1.5°C warming target. The deep Pacific shows increased O_2 compared to PI, while most of the upper ocean shows less O_2 compared to PI (as a result of less solubility). As the AMOC recovers to PI values, anomalies are less pronounced in the Atlantic Ocean.

Page 12 lines 30-31: I dont agree with the statement Proxies of past ocean oxygenation and ventilation reveal similar structural changes and mecha- nisms. 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).

We thank the reviewer for sharing his insight and accordingly remove the paleo discussion from the manuscript.

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