Reviewer 3

*Overall summary and appreciation*

*Duboc et al. present a study on the simulated oxygenation in previous interglacials where their focus is on a comparison of the MIS9e and Last Interglacial (LIG) to the modern (pre-industrial; PI) state. The ACCESS-ESM1.5 model that they employ for their study is a fully coupled atmosphere-ocean model that also considers biogeochemistry in the ocean towards resolving the carbon cycle and related processes that have an effect on oxygenation. The authors present several findings that show that oxygenation of sea water may have looked quite different from today during past interglacials. For me personally, the sensitivity of oxygen availability in the Mediterranean Sea, and in particular the differences that the authors find in this region for the two different interglacials, is the most impressive result. If the authors shared my point of view then I would invite them to reflect in their discussion on some aspects that I suggest below.*

We would like to thank the Reviewer for this judgment, and we agree.

*The study is certainly of relevance for a wider audience. Understanding the mechanisms that may have impacted on the distribution and extent of oxygen minimum zones during past warm climates may give us an idea of the respective changes that we are currently already facing or that we may have to expect in the future. The question of whether greenhouse gas levels or insolation differences are the most relevant drivers of oxygenation is key in this effort. The authors seem to find that greenhouse gases are less relevant than insolation. I am asking myself whether this statement could be made in a more quantitative manner based on the simulation data at hand, or whether doing so would involve additional simulations that consider a separation of forcings.*

Unfortunately, a more quantitative approach would require additional sensitivity simulations which would be very expensive and long to run. We would also like to point out that this statement is only valid for the relatively small changes in greenhouse gas forcing between these three interglacials. We have now rephrased this statement to:

"**The large-scale ocean circulation patterns, including AABW, NADW and the ventilation of the North Pacific Ocean are therefore very sensitive to the latitudinal and seasonal distribution of incoming solar radiation in the ACCESS ESM1.5, and less sensitive to changes in greenhouse gas concentrations within the range of these three interglacials.**"

*I have read the manuscript with great interest and find it generally worth to publish as a research article in Climate of the Past. The text is of high quality, but I also note that graphical presentation of results can still be significantly improved. I would like to ask the authors to also reconsider the overall structure of the article that currently presents various important results in a very extensive appendix.*

We agree and we have made the requested changes (see our responses below).

*Please note that while I am a climate modeller, I do not have lots of expertise in sea water oxygenation and in marine biogeochemical cycles. I apologize therefore to the editor and to the authors if I cannot provide very deep insights on these topics.*

*Please find below various comments that I suggest for consideration towards a revised manuscript.*

*General comments:*

*I find the results on different oxygenation of the Mediterranean Sea depending on the considered time slice remarkable. In the Mediterranean Sea, the three different interglacials considered (PI, LIG, MIS9e) appear to show completely different response to climate state and boundary conditions. Enhanced stratification (mostly due to reduced surface salinity that is a result of enhanced river runoff to the Mediterranean Sea) certainly contributes to this effect. Could the authors highlight in their discussion differences in the sapropel records between LIG and MIS9e?*

That is an excellent idea, but unfortunately there isn't that much work that compares the different sapropels in the Mediterranean either qualitatively or quantitatively in terms of oxygenation, hydrology, primary productivity. There are suggestions that the transition from dry to wet conditions during sapropel events was different between S5 (LIG) and S10 (MIS 9e), with the latter changes being rapid compared to progressive under S5 (Melki et al., 2010), but whether this influenced seawater oxygen contents is unclear.

*Sapropels are already summarized in the manuscript, but rather with a focus on different regions than on different time periods. Simulation results shown in Figure 7d indeed show that the Mediterranean Sea is in LIG and MIS9e for most of the year not evaporation controlled, which is the current mode of control, but rather runoff- or preciptation-controlled. Nevertheless, the difference in oxygenation between MIS9e and LIG (Fig. 5c) seems large in comparison to the vertical profiles for temperature, salinity, and water mass age that are shown. Is there a strong non-linearity at play inside the physical / circulation system that could explain this effect, or are there other effects that contribute (e.g. different respiration / activity by simulated organisms in the various interglacials)? Can you make respective statements based on the results derived from the biogeochemistry component of your climate model?*

The rate of change in $O_2$ is similar for LIG and MIS 9e (new Figure 1b), although a bit steeper for the LIG simulation. The main difference in oxygenation between these two simulations is therefore due to the fact that the LIG stratifies earlier and has spent more time in a stratified state (please also see our response to the next comment).

However, there are indeed differences in biological productivity between LIG and MIS 9e which could explain the difference in the slopes in Figure 1b. Fig R3.1. shows that phytoplankton concentrations in the MIS 9e simulation are smaller than in the LIG simulation, especially in the Sea of Sicily. This is due to higher nutrient availability (Fig R3.2.) in the LIG simulation which is caused by slightly enhanced mixing off the coast of North Africa.

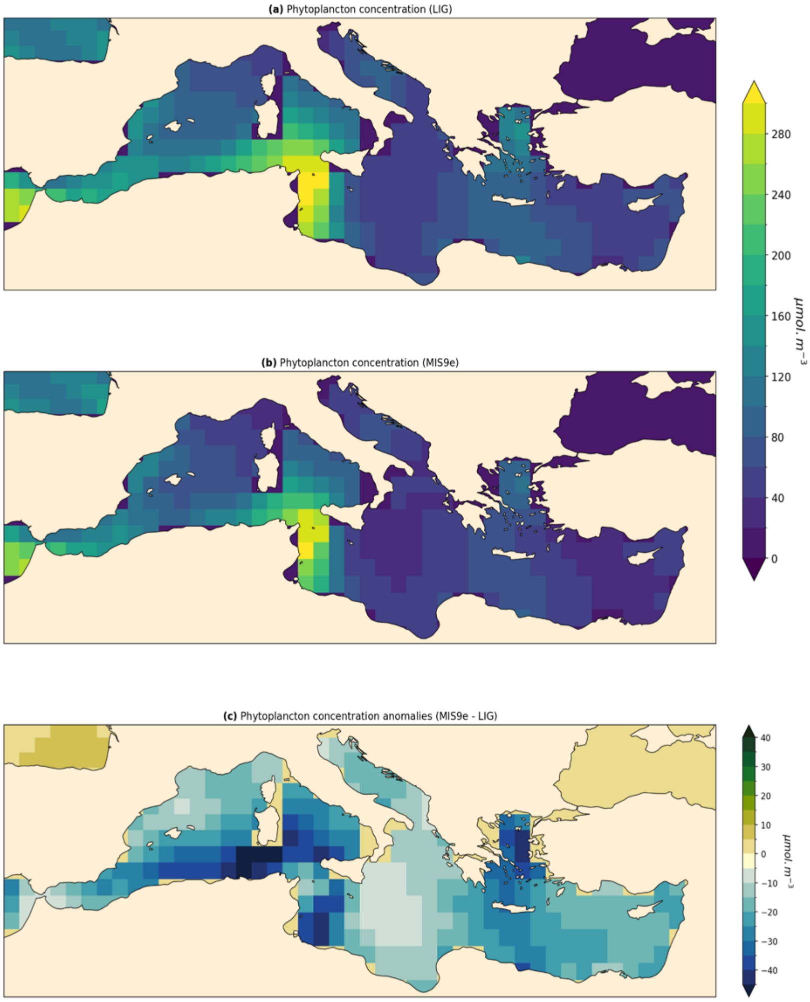


*Figure R3.1. Phytoplankton concentration averaged over the top 50 m in the Mediterranean Sea for (a) LIG, (b) MIS 9e, (c) MIS9e-LIG.*

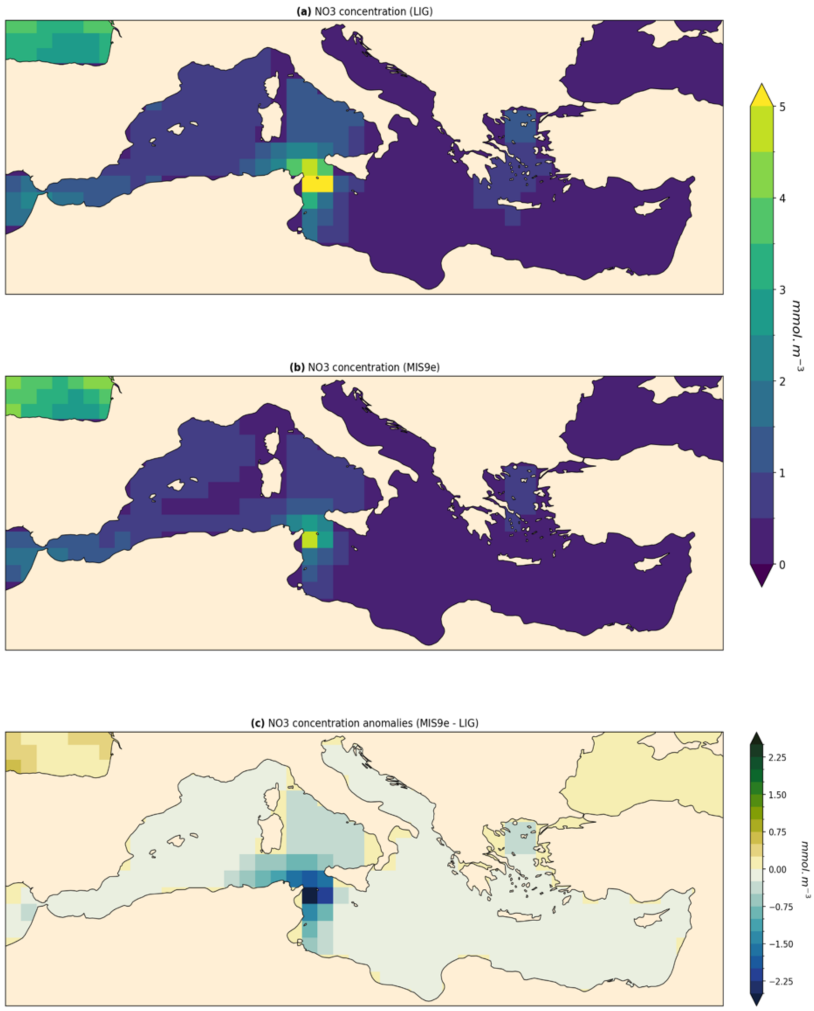


*Figure R3.2. Nitrate concentration averaged over the top 50 m in the Mediterranean Sea for (a) LIG, (b) MIS 9e, (c) MIS9e-LIG.*

Ocean temperatures are comparable for these simulations in the Mediterranean Sea (new Figure 8a), so the temperature-dependent remineralisation rates should be comparable.

We have added following sentence to Section 3.2:

"**Sea surface salinities decrease at a faster rate and lead to stratification and deoxygenation earlier than in the MIS 9e simulation (Figure 1b). The difference in oxygenation between LIG and MIS 9e is thus mainly due to the difference in the length of time during which the Mediterranean Sea is stratified, although small changes in biological productivity and export production (not shown) also contribute. It should be noted that oxygen levels in the Mediterranean Sea are still drifting in both simulations and that the equilibrium values are lower than what is presented here.**"

*Around line 300 of your manuscript you state that imperfect equilibration could be a cause for differences that you find between LIG and MIS9e. Can you give quantitative measures that support this statement? Integration lengths (Table 1) seem rather similar for both simulations. Do you assume that the difference in initial states, or differences in the equilibration times that arise from differences in the forcings, could be the explanation?*

New Figure 1b shows the time series of oxygen content in the Mediterranean Sea for the three simulations. It can be seen that the LIG and MIS9e simulations follow the PI control simulation closely for the first ~300 years. At around ~300 years, oxygen starts to decline in the LIG simulation. The increased river runoff therefore needs a few hundred years to accumulate before the Mediterranean Sea stratifies and oxygen at depth starts to decline. The same is true for the MIS9e simulation, but it branches off later, after ~500 years. This is due to the fact that the monsoon is not quite as strong as in the LIG simulation (new Fig 10). It therefore takes longer for the Mediterranean Sea to stratify in the MIS9e simulation than in the LIG simulation. Once stratified, oxygen declines at an almost linear rate in both simulations, although the LIG simulation shows some signs of leveling off, nearing equilibration.

It is unfortunately impossible to give a quantitative statement on where the MIS9e simulation will level off without extending the simulations.

*On the other hand, Figure A11, right column, seems to suggest (although difficult to read) that, while there is more Apparent Oxygen Utilization (AOU; -AOU shown in the graphs, right?) in LIG and MIS9e than in PI, Last interglacial may see much more utilization of oxygen than MIS9e (I hope that I have correctly applied the factor -1 in this argument). AOU would be due to bioactivity and decomposition of organic material, right? Therefore, some process(es) from the biogeochemistry subystem of the Earth seem to contribute to the difference in oxygenation between LIG and MIS9e, am I right? Could you discuss in more detail what these contributions could be?*

The AOU is calculated as the difference between saturated $O_2$ and in situ $O_2$. It is therefore a diagnostic of oxygen consumption. For a particular water parcel, this consumption will depend on the history of this water parcel since it has last "seen" the atmosphere. It will be the integral of remineralisation rates during its journey. Changes in AOU can therefore be due to:

(a) changes in the availability of organic matter to remineralise on the pathway of this water parcel (i.e. changes in export production),

(b) changes in temperatures on the pathway of this water parcel (i.e. remineralisation will increase with higher temperatures due to higher metabolic rates), and, most importantly,

(c) changes in circulation (i.e., if circulation weakens and residence times increase, the integral of remineralisation rates since last contact with the atmosphere will increase).

For the Mediterranean Sea, these large increases in AOU are mainly due to changes in circulation. They would be expected to be higher for the LIG run than for the MIS9e run, because the LIG run stratified earlier in the simulation, and the stratification is stronger.

Biological productivity and export production also plays a role, as biological productivity is slightly reduced at MIS 9e compared to LIG (Fig R3.1.c). It is unfortunately impossible to disentangle these processes in the current modelling frame.

As mentioned above, we have added following sentence to Section 3.2:

"**Sea surface salinities decrease at a faster rate and lead to stratification and deoxygenation earlier than in the MIS 9e simulation (Figure 1b). The difference in oxygenation between LIG and MIS 9e is thus mainly due to differences in the length of time during which the Mediterranean Sea is stratified, although small changes in biological productivity and export production (not shown) also contribute. It should be noted that oxygen levels in the Mediterranean Sea are still drifting in both simulations and that the equilibrium values are lower than what is presented here.**"

*Analysis at orbital time-scales: Are seasonal averages shown in Figures A7, A8, and A13 based on a modern calendar or are seasons corrected for different orbital configuration? If your Fig. A1 is taken as a reference then I assume the calendar is modern (refer to Figure 3 by Otto-Bliesner et al., 2017). If so, please comment on potential implications for your results.*

All seasonal plots have now been replotted with the LIG adjusted calendar. However, we have not changed Figure A1 because we prefer a linear scale for the x-axis of this Hovmöller diagram. The x-axis of A1 therefore remains as "day of the year" without artificially expanding or contracting time due to differences in lengths of different months.

*Volume of the appendix in comparison to volume of the manuscript: My observation is that the extent of figures in the appendix (13 figures) is quite large and far beyond the number of figures in the actual manuscript body (7 figures). Various figures in the appendix seem important to the message of the manuscript (in particular Figures A3-A6), and in my humble opinion some of the figures could be considered to present main results of the work by Duboc et al. (in particular A11 and A12). The extensive appendix makes reading the manuscript cumbersome at times, with the need to repeatedly scroll back and forth. I suggest to check whether there is a better way to integrate at least the most important results into the main manuscript text, thereby reducing the size of the appendix.*

We have now moved Figures A2, A3 and A5 to the main text.

*Presentation of results in figures: I think much more work can be invested in improving figures with regard to clarity and readability. In most cases fonts are far too small to be even barely readable on a normal A4 printout (at least for a person with my eyesight). Proportions of graphical elements are sometimes out of* *scale (e.g. the vertical spatial extent of the colorbar at Fig. 4 covers about a third to a half of any of the individual figures, which is quite a lot, while, nevertheless, neither the colorbar labels nor the tick labels are readable due to too small font size). The text width of the manuscript, that is available to present graphical results, could be much better utilized by optizing the spacing between individual figure elements (e.g. by reducing the space between left and right columns of Figure 4 and by extending the width of the whole figure panel to cover all useable space of the text width).*

*Lots of text within figures: As stated above, it is absolutely necessary to increase the font size of text elements in figures across the manuscript. This will of course lead to problems with extensive text information that is included for example as headers of subfigures. I suggest to critically evaluate which part of the information that is currently presented as subfigure heading or annotation must be presented as part of the figure panel, and which part of the information can be savely moved to a figure caption to reduce complexity in the figures themselves.*

This comment was mirrored by Reviewers 1 and 2. We have now changed all the figures. In particular, we have:

- Removed the titles of the subplots;
- Made the legend colour bars thinner where appropriate;
- Increased the font size for all remaining text in the figures;
- Reduced the white space between subpanels where possible.

*Spaces between physical units and preceding numbers: In particular for the unit meters there seems to be a more or less consistent lack of spacing between the number and the symbol „m“, both in the main text, in Tables, in figure captions, and potentially also in annotations in figures themselves (that I cannot always read). Please check spacing and correct where necessary (e.g. in Table 1, in lines 116, 150, 161, 164, 166, captions of Fig. 1, 3, 4; this list is not exhaustive).*

This has been fixed.

*Results on water masses properties, lines 120ff: I was wondering whether it would be sensible to add the main result regarding oxygenation to the title, or to alternatively present it as a clear statement at the end of the respective subsection. Doing so would guide the reader through the most relevant outcomes of your simulations. For example, section title 3.1.2. could become „North Atlantic Deep water - colder and more ventilated“.*

We have changed the subtitles as follows:

**“3.1.1 Antarctic Bottom Water - warmer and less ventilated”**

**“3.1.2 North Atlantic Deep Water - colder and better ventilated”**

**“3.1.3 North Pacific Intermediate Water - warmer and better ventilated”**

**“3.1.4 Oxygen Minimum Zones (OMZs) - no significant change”**

**“3.2 Oxygenation of the Mediterranean Sea - large-scale hypoxia”**

*The metric AOU: Please note that I am not an expert in ocean oxygenation, so I am not very familiar with this metric. Would it be sensible for readers of my low level of expertise to spend a few sentences on the mechanisms that contribute to AOU in reality and in your model? Could you also address in the discussion of your manuscript the differences between AOU and a newer metric True Oxygen Utilisation (Duteil et al., 2013) and outline, to which extend the use of one or the other metric in your study would impact inferences with regard to modelling output or any model-data comparison?*

This was also flagged by Reviewer 2 and we apologize for the omission. Following paragraph has been added to the end of Section 2:

**"In Section 3 we partition changes in dissolved oxygen into two components, changes in the saturated concentration of oxygen $O_2^{sat}$ and changes in Apparent Oxygen Utilisation (AOU). AOU estimates the oxygen consumed during respiration and can be calculated as the difference of dissolved oxygen concentration ($O_2$) and $O_2^{sat}$:**

$$AOU = O_2^{sat}(T, S) - O_2 \qquad (1)$$

**where T is the potential temperature and S salinity. Changes in AOU are therefore a combination of changes in circulation (with sluggish water masses tending to have higher AOU), and changes in remineralisation rates, which depend on the vertical export of organic matter (export production) and temperature. Please note that the here used metric AOU assumes that dissolved oxygen in surface waters is in equilibrium with the atmosphere, which might lead to an overestimation of the True Oxygen Utilisation (TOU) (Duteil et al., 2013)."**

*Specific comments:*

*Line 20: should „ocean" be replaced by „oxygen"?*

Nice catch! Thank you.

*Line 23: model-dependent*

"dependent" is correct in American spelling, whereas "dependant" is correct in British and Australian spelling. Given that we are Australian, we left the text in Australian (British) spelling for now - and Climate of the Past can then change the spelling as they see fit according to their publication guidelines.

*Line 79-83: Here I got a bit lost in the formulation. Phytoplankton and zooplanktion represent functional components of the biological system while the aspects described thereafter rather refer to prognostic tracers, is my understanding correct? Would reordering the text as follows improve readability while still conveying correct information? „It includes one functional type of phytoplankton and zooplankton. As prognostic tracers it simulates dissolved inorganic carbon (DIC), alkalinity (ALK), phosphate (PO4), oxygen (O2), and iron." Thereafter, reordering may again improve clarity of formulation: „Detrital decomposition is a function of temperature and is allowed to occur when oxygen is zero. Even though nitrification and denitrification are not explicitly included in the model and the global nitrogen budget is kept constant (Oke et al., 2013), this formulation emulates the effect of denitrification."*

We have made the suggested changes, although phytoplankton and zooplankton are also prognostic tracers.

The paragraph now reads:

**"WOMBAT is a nutrient-phytoplankton-zooplankton-detritus (NPZD) model (Oke et al., 2013; Law et al., 2017; Ziehn et al., 2020). It includes one functional type of phytoplankton and zooplankton. As prognostic tracers it simulates dissolved inorganic carbon (DIC), alkalinity (ALK), phosphate ($PO_4$), oxygen ($O_2$), and iron. The stoichiometry is fixed at a C:N:P:$O_2$ ratio of 106:16:1:-172. $CaCO_3$ export from the photic zone is set at ~8% of the organic carbon export. Detrital decomposition is a function of temperature and is allowed to occur when oxygen is zero. Even though nitrification and denitrification are not explicitly included in the model and the global nitrogen budget is kept constant (Oke et al., 2013), this formulation emulates the effect of denitrification. The dissolution of $CaCO_3$ occurs at a constant rate. All organic and inorganic particles reaching the bottom are remineralized, given that ACCESS-ESM1.5 does not include burial of sediments."**

*Line 84: „All organic and inorganic particles reaching the bottom are remineralized, given that ACCESS-ESM1.5 does not include burial of sediments". Am I right in my assumption that the fact that carbon does not exit the ocean system at the lower boundary, but is instead fed back to the ocean via remineralization, may have an impact on simulated oxygenation of sea water? Please kindly refute or confirm my assumption.*

Yes, the Reviewer is correct.

*If you can confirm my assumption, then please explain (e.g. in your discussion) whether the effect of remineralization on oxygenation is relevant for the inferences that you derive from your study. In particular, is the effect comparable across different climates, so that the observed differences in oxygenation are fully attributable to a difference in climate states rather than a side-effect of the missing sediment module (that may be more (or less) important in dependence of the background climate state).*

In the real ocean, and in ocean models that include sediment modules, only a very small fraction of the tracers sinking to the ocean bottom is ultimately buried, while most are remineralised back into the water column. While we cannot quantify the effect of a missing sediment model on oxygen concentrations with the current modelling frame, we expect it to be very small. A potentially larger impact on oxygen patterns would be the nutrient exchange at the sediment-ocean interface, especially in oxygen-deprived regions (e.g. Niemeyer et al., 2017), but these processes are not included in any of the (rare) global Earth System Models with sediment components.

*Line 86: Since Eyring et al. (2016) describe the CMIP6 settings, maybe reformulate: „The pre- industrial 1850 equilibrium simulation (PI) is integrated following the CMIP6 protocol (Eyring et al., 2016) with the exception of using the CMIP5 solar constant (1365.65 W.m−2)."*

This has been amended.

*Line 107: Am I right that a „N/S" should be added after „40°"?*

We added "N/S" after "40°".

*Line 109: Also here please provide the reference(s) (unless Table 1 now clearly lists them).*

We have now added the references to the caption of Table 1. The caption reads now:

"**Table 1. Experimental set-up. LIG boundary conditions follow PMIP4 protocol (Otto-Bliesner et al., 2017). MIS 9e boundary conditions are based on Berger (1978) for orbital parameters, and peak concentrations for methane (Loulergue et al., 2008), carbon dioxide (Bereiter et al., 2015), and nitrous oxide (Schilt et al., 2010).**"

*Line 123-124: „This weakening of deep-ocean convection is mainly due to reduced sea-ice formation" - differences in sea ice formation will impact deep-ocean convection both with regard to the amount of brine rejection and the intensitiy of atmosphere-ocean coupling. Furthermore, presence or absence of sea ice will likely also modulate the atmosphere-ocean gas exchange. Can it be quantified or estimated which of these effects is more relevant?*

No, this is unfortunately not possible in a fully coupled ESM.

*Line 126: Please define Apparent Oxygen Utilisation. I am not sure whether all readers are familiar with this metric.*

This is now done in the Methods section, please see our reply above.

*Line 130: „with the increase in export production and higher remineralisation rates, and secondarily <u>caused by</u> the temperature-depended solubility"*

This has been amended.

*Line 133: „Figure 2a and d <u>show</u> that"*

This has been fixed.

*Line 144: Split the long sentence: „subtropical saline waters. Both are" Line 145-146: use the abbreviation NADW*

This was changed.

*Line 168: „at the surface (not shown), <u>that is linked to</u> lower primary <u>productivity and</u> export production"*

Changed to:

**"... at the surface (not shown), causing lower primary productivity and export production"**

*Line 180: Has significance been tested? If so consider to show this in the figures, e.g. via hatching?*

This was also flagged by Reviewer 1. We have now replotted all figures showing anomalies in a way that clearly highlights regions that are not significant.

*Line 190: For clarity, consider to move the statement „in the LIG simulation" to the start of the sentence.*

This was done.

*Line 215: Add commas for clarity of the formulation: „in the MIS9e simulation, dropping to 52.4 mmol m−3,"*

We added commas.

*Line 223: add a space: „128 ka BP", same at line 224 (and potentially at other locations)*

This was amended and checked for consistency throughout the text.

*Line 289: try to avoid the word segmenta of MIS 9e being separated by line breaks (maybe do not use a space in the term, across your manuscript?)*

This has been fixed throughout the text.

*Line 302: This region remains*

Fixed.

*Line 308: please check whether „dependent on" or „dependent of" is grammatically correct here (I am not native English speaker)*

"Dependent on" or "dependent upon" is correct. This has been fixed. Thank you.

*Line 312: long equilibration times?*

Fixed.

*Line 317-319: Is your statement regarding higher sensitivity to insolation than to greenhouse gases robust if also taking into account the differences in oxygenation found between interglacials in the Mediterranean? I think that a strict discrimination between different sensitivties and estimation of their relative strength would be only possible based on simulations that include separations of forcing, am I right?*

Yes, the reviewer is correct. A strict discrimination between different forcings would only be possible with additional sensitivity simulations. We believe, however, that our statement is correct, given that the large-scale circulation and oxygenation in the MIS 9e and LIG simulations are very similar and they are both very different from PI. The differences in the Mediterranean Sea between MIS 9e and LIG are mainly due to a combination of differences in monsoon strengths (which are mostly due to orbital parameters) and equilibration times.

*Line 330: „and 63% is hypoxic"*

Fixed.

*Line 335: consider the spelling of skillful, at least at one other location you spell it with one „l"*

Skillful is spelled with two l in American English and with one l in British/Australian English. We have now adopted British spelling of skilful throughout the text.

*Specific comments to Figures (beyond font size, which represents a problem across the manuscript) and Tables:*

*Table 1: Please provide the reference to the parameter values provided, in particular orbital parameter solution and reference for greenhouse gases. While on may guess that Otto-Bliesner et al. (2017) is the reference for PI and LIG, providing the citation is particularly important for MIS 9e where there is no obvious reference that comes to my mind. Such information may also be used to make the table heading a bit more informative. Regarding footnote 1: How, if at all, does erroneous forcing affect results? It is appreciated that such information is conveyed to the reader. Nevertheless, a bit of evaluation on the potential impacts (or the absence of such) may be of interest to the readers.*

We have now amended the caption of Table 1 (see above).

The first 372 years of our LIG simulation were accidentally integrated with PI greenhouse gas concentrations (and LIG orbital parameters). This does not affect the quasi-equilibrium state that is analysed here as the simulation was then equilibrated for a further 1450 years with the correct boundary conditions.

We have now clarified the footnote of table 1:

**"The first 372 years have erroneous forcing and were integrated with PI greenhouse gas concentrations."**

*Figure 2: Please define AOU in the caption (also in other Figure captions, where needed). Contour lines and contour line labels carry little information for me since I cannot read them.*

We have now spelled out AOU in all Figure captions and added a description of AOU to the Methods section (see above).

*Figure 4: Differences in black contour lines (solid, dotted, dashed) not visible to me. Furthermore, I am not sure about usage of footnotes in figure captions. I would avoid them and rather implement the information directly as caption text.*

These contour lines are almost identical and therefore only visible when zooming in (we still need to show them though). This is stated in the text (this Figure is now Figure 7):

**"The extent and intensity of OMZs is very similar between the three simulations at 300 m depth (Figure 7a and c)."**

The figure caption has been changed to:

**"$O_2$ concentration and anomalies at 300 m depth in hypoxic zones (left) and vertical minimum of $O_2$ concentration and anomalies in hypoxic zones (right) in (a, b) LIG - PI, (c, d) MIS 9e - PI, (e, f) PI and (g, h) World Ocean Atlas (WOA, 1965-2022). Black contour lines in subplots a-d indicate the 62 mmol·m$^{-3}$ isolines for PI (solid), LIG (dotted), and MIS 9e (dashed). Hypoxic zones are defined as zones where $O_2$ concentration at 300 m is below 62 mmol·m$^{-3}$ for (a, c, e, g) and where the vertical minimum of $O_2$ concentration is below 62 mmol·m$^{-3}$ for (b, d, f, h)."**

*Figure A1: Show hovmöller plots with calendar corrected data, unless there is a good reason to use the modern calendar.*

Our Hovmöller plot has an x axis that shows the day of the year, with each day weighted equally. When changing this to months (either for present day calendar or LIG calendar), as often done in publications, the plot becomes distorted and shorter months are over-represented. This is particularly true for the LIG adjusted calendar which has a larger range in month' lengths than the present day calendar. Our Hovmöller plot therefore shows neither modern day nor LIG calendar - just the 365 days of the year.

*Figure A2: It is difficult for me to extract any meaningful information from the very small legends, boxes, and thin lines. Keeping this in mind, it is also difficult for me to identify any interpolated data that you refer to in the caption.*

Figure A2 (new Figure 1) has been replotted.

*Figure A5: Can you explain the artifact at about 20°N in the AMOC (left column)? My first guess would have been the Strait of Gibraltar, but fluxes across the gateway are probably too small and the location of the artifact also does not fit.*

We would like to thank the Reviewer for catching this. This artifact was due to a small inconsistency in the mask used for the Atlantic Ocean. This has now been fixed.

*Figure A6: There is stronger AMOC in LIG and MIS9e (Fig. A5) but the mixed layer depths are smaller in these simulations than they are in PI; i.e., there is a weaker link between AMOC strength and mixed layer* *depth in MIS9e and LIG. Does this difference in dynamics contribute to your findings regarding oxygenation?*

This is actually not quite correct, the mixed layer depths are not smaller in these simulations than they are in PI. When looking at Figure A6 (new Figure A3), one can see that a new convection site is active in the Labrador Sea in the MIS 9e and LIG runs. This new convection site contributes colder water to NADW and is responsible for the increase in ventilation. All other convection sites remain active in all three simulations, with the convection site south of Spitsbergen being weaker in the MIS9e and LIG runs compared to PI. We assume that the Reviewer was referring to that site? Overall, the new convection site makes up for this weakening.

*Furthermore, you mention in the main text that changes in deep mixing are linked to different sea ice conditions. So, can the weakened deep convection in the Barents Sea near Svalbard be explained by less autumn-to-winter sea ice formation and the related reduced brine rejection?*

The Reviewer is referring to following sentence in “Section 3.1.1 Antarctic Bottom Water”:

**“This weakening of deep-ocean convection is mainly due to reduced sea-ice formation (Yeung et al., 2024; Choudhury et al., 2022) and leads to warmer, less ventilated, and therefore less oxygenated AABW.”**

This sentence referred to convection in the Southern Hemisphere, which was analysed in detail in our group in earlier publications. We have not yet analysed the regional dynamics in the North Atlantic for these simulations, and it would be out of scope to do so for this manuscript.

*Figure A8: Would it make sense to also show the PMOC to understand the links between deep ventilation and meridional overturning?*

These rather small changes in ventilation unfortunately do not show up on stream function plots of the Pacific Ocean. That’s why we opted to show the changes based on changes in mixed layer depth (old Figure A8, new Figure A5) and water age (old Figure A9, new Figure A6).

*Figure A9: Is there a reason why you compare in a and b different depths with each other (250 m vs 50 m)?*

We apologize, that was a typo. It was meant to read 250 m for both subplots.

*Figure A11: define the abbreviation AOU*

Done.

*References:*

*Eyring, V., Bony, S., Meehl, G. A., Senior, C. A., Stevens, B., Stouffer, R. J., and Taylor, K. E.: Overview of the Coupled Model Intercomparison Project Phase 6 (CMIP6) experimental design and organization, Geosci. Model Dev., 9, 1937–1958, https://doi.org/10.5194/gmd-9-1937-2016, 2016.*

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