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.

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.

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.

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? 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? 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? 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?

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.

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.

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.

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).

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“.

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?

Specific comments:

Line 20: should „ocean“ be replaced by „oxygen“?

Line 23: model-dependent

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.“

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. 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).

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).“

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

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

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?

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

Line 130: „with the increase in export production and higher remineralisation rates, and secondarily caused by the temperature-depended solubility“

Line 133: „Figure 2a and d show that“

Line 144: Split the long sentence: „subtropical saline waters. Both are“

Line 145-146: use the abbreviation NADW

Line 168: „at the surface (not shown), that is linked to 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?

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

Line 215: Add commas for clarity of the formulation: „in the MIS9e simulation, dropping to 52.4 mmol m^{-3},“

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

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?)

Line 302: This region remains

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

Line 312: long equilibration times?

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?

Line 330: „and 63% is hypoxic“

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

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.

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.

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.

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

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 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.

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? 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?

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

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

Figure A11: define the abbreviation AOU

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.

Otto-Bliesner, B. L., Braconnot, P., Harrison, S. P., Lunt, D. J., Abe-Ouchi, A., Albani, S., Bartlein, P. J., Capron, E., Carlson, A. E., Dutton, A., Fischer, H., Goelzer, H., Govin, A., Haywood, A., Joos, F., LeGrande, A. N., Lipscomb, W. H., Lohmann, G., Mahowald, N., Nehrbass-Ahles, C., Pausata, F. S. R., Peterschmitt, J.-Y., Phipps, S. J., Renssen, H., and Zhang, Q.: The PMIP4 contribution to CMIP6 – Part 2: Two interglacials, scientific objective and experimental design for Holocene and Last Interglacial simulations, Geosci. Model Dev., 10, 3979–4003, https://doi.org/10.5194/gmd-10-3979-2017, 2017.

Duteil, O., Koeve, W., Oschlies, A., Bianchi, D., Galbraith, E., Kriest, I., and Matear, R.: A novel estimate of ocean oxygen utilisation points to a reduced rate of respiration in the ocean interior, Biogeosciences, 10, 7723–7738, https://doi.org/10.5194/bg-10-7723-2013, 2013.