Answers to referees for the submitted manuscript

Evolution of oxygen and stratification in the North Pacific Ocean in CMIP6 Earth System Models

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Referee #1

Summary:

While the topic is certainly relevant, the approach used is novel and the motivation of the study is clearly justified, in my opinion this manuscript is only marginally suitable for Biogeosciences as its focus is mostly on the physical processes responsible for the supply of oxygen to the upper ocean. I therefore think that this manuscript is much more suitable for Ocean Science (also a Copernicus journal; see subject areas there). This does not however diminish the merit of this manuscript which upon moderate revision should be apt for publication in the appropriate journal.

While we understand the reviewer's concerns, we respectfully disagree. The topic, oxygen reconstruction, is of interest to ocean biologists far more than to physical oceanographers, and the readership of Biogeosciences represents the one we wish to reach. Using the words of reviewer 2: "The application to O2 is highly relevant as observations of oxygen are becoming more autonomous and is interesting on both physical and biogeochemical fronts because of its relationship to ventilation processes as well as ecosystem health."

General comments

Strengths:

The manuscript is mostly well written and the presentation of the results is of high quality. Except for a few areas that need clarification (see specific comments below), the manuscript provides an accurate account of the approaches followed by the authors to carry out their analyses. Generally speaking, the text is well structured and the connection between objectives, approach, results and the corresponding conclusions is clear and easy to follow.

We thank the reviewer for the overall positive assessment of our work.

Weaknesses:

- "After reading the manuscript it is not completely clear what is the level of uncertainty associated with the projections carried out with the different models (described as "future" in the text and figures). This should be clearly stated as it might impact the applicability of the suggested approach if it were to be employed (as suggested by the authors) with large-scale data from autonomous platforms."

  We state in the revised manuscript that the existing uncertainty in CMIP6 models' oxygen inventory changes poses a limit to the information that may be extracted in the future. While in some models the relationship between IPV* and O2 becomes stronger, that is not the case for all of them, and in particular for the GFDL model which has a better representation of the historical period than the others. We additionally point to the recent work of Abe and Minobe (2023) for a detailed model intercomparison of ocean deoxygenation in CMIP6 models, which was beyond the scope of our work. Additionally, we now better clarify that a major objective of this study is the presentation of a novel integrated approach to assess some of the major relationships between climate dynamics, upper ocean stratification and oxygen inventory, and to show if/when these relationships are robust across some CMIP6 models, despite their inherent uncertainties. We also add additional literature to better frame the limitations of our results (besides Abe and Minobe

(2023), we include Tokarska et al (2020), Beobide-Arsuaga et al (2021) and Kwiatkowski et al (2020).

- "While the study covers open and coastal ocean areas, from the plots it is evident that there is a limitation on the resolution in close-coastal areas. I think it would be worth stating the proximity to the coasts at which these analyses are valid, as biogeochemical processes affecting the oxygen distribution in e.g. eastern boundary upwelling systems are particularly intense within 50-150 km from the coast (do we expect the variability in near-coastal areas to be smoothed out in climatological time scales?)"

  Our work covers open ocean and coastal areas but the currently available resolution for CMIP6 models (or the publicly available observations for most part) does not allow for a detailed representation of the fine-scale (mesoscale and finer) physical and biogeochemical processes occurring near the coast except for specific coastal systems (for example the California Current System). While we acknowledge that this is a limitation, and an important one in the description of, for example, eastern boundaries upwelling systems (EBUS) biogeochemistry, our conclusions focus on large scale processes occurring in extended areas of the open ocean rather than on the coastal zones alone. In the North Pacific observations and models both suggest that there may be a large-scale modulation of oxygen content that extends to the coast. In addition, and most importantly, the main findings for the hotspots analysis, such as the large scale longitudinal de-coupling of hotspots and the extratropical location of changes, are unlikely to be influenced by the models' resolution approaching the coast.
  In the revision, we acknowledge that O2 concentrations in coastal areas are driven by complex biophysical interactions and physical processes unresolved by state-of-the-art climate models. Consequently, projected oxygen trends may exhibit variability even within subregions under the same scenario (see, again for EBUS, Bograd et al. (2023)). Analysis at the scales required to capture coastal dynamics, however, would require higher resolution models that will use - if projected into the future – CMIP6 runs as boundary conditions. Our work would remain relevant in the interpretation of the large scale forcing, while the overall methodology could be applied broadly even in higher resolution models/observations. We think it is important to clarify these aspects in the manuscript, and we better explain both limitations and applicability of our work in the revised version. We thank the reviewer for highlighting this point.

- "The results and conclusions section are rather puzzling. While the results in all subsections are well described, the discussion is limited and at the end one does not grasp the main message until reading the whole manuscript. My impression is that most of the current text on the conclusions actually corresponds to the missing text on the discussion. I recommend the authors to revise these two sections and cut down the conclusions to a brief text in which they state whether the study's goals (as laid out in the introduction) were achieved or not and why."

  We thank the reviewer for this comment, which we comply with by restructuring the discussion and conclusion sections of the manuscript as suggested.

- Specific comments (All taken care of)

l.122: replace "for which" by "in which".
l.143: replace "its" by "theirs"
l.235: I strongly suggest changing this section to "Results and Discussion".
l.243–244: Please elaborate on the criteria used to classify an area as "most impacted by ENSO".

l.252: "(…) where the variability is dominated by PDO (…)". Same comment as above for ENSO.
l.337–340: This sentence is long and not understandable at all. Please revise.
l.338: Remove apostrophe after "Peru".
l.375–378: This sentence is long and not understandable at all. Please revise.
l.426–437: This can be omitted or significantly reduced as this information is mostly redundant.
l.474: Consider changing to: "pointing to an area of further investigation".

We made all the requested minor corrections. We also clarify that we identified the ENSO and PDO impacted areas using the network inference and that we referred to the δ-MAPS domains in Figures 4-5.

- General comment to tables: according to Copernicus guidelines, "horizontal lines should normally only appear above and below the table, and as a separator between the head and the main body of the table". Please revise.

  We changed the table as suggested. Thanks for noticing it.

References used in the answer to Referee#1:

Abe, Y. and Minobe, S.: Comparison of ocean deoxygenation between CMIP models and an observational dataset in the North Pacific from 1958 to 2005. Frontiers in Marine Science. 10. 10.3389/fmars.2023.1161451, 2023

Beobide-Arsuaga, G., Bayr, T., Reintges, A., and Latif, M.: Uncertainty of ENSO-amplitude projections in CMIP5 and CMIP6 models, Clim. Dynam., 56, 3875–3888, https://doi.org/10.1007/s00382-021-05673-4, 2021.

Bograd, S. J. et al.: Climate change impacts on eastern boundary upwelling systems. *Annu. Rev. Mar. Sci*. **15**, 303–328, 2023.

Kwiatkowski, L., Torres, O., Bopp, L., Aumont, O., Chamberlain, M., Christian, J. R., Dunne, J. P., Gehlen, M., Ilyina, T., John, J. G., Lenton, A., Li, H., Lovenduski, N. S., Orr, J. C., Palmieri, J., Santana-Falcón, Y., Schwinger, J., Séférian, R., Stock, C. A., Tagliabue, A., Takano, Y., Tjiputra, J., Toyama, K., Tsujino, H., Watanabe, M., Yamamoto, A., Yool, A., and Ziehn, T.: Twenty-first century ocean warming, acidification, deoxygenation, and upper-ocean nutrient and primary production decline from CMIP6 model projections, Biogeosciences, 17, 3439–3470, https://doi.org/10.5194/bg-17-3439-2020, 2020

Tokarska, K. B., Stolpe, M. B., Sippel, S., Fischer, E. M., Smith, C. J., Lehner, F., and Knutti, R.: Past warming trend constrains future warming in CMIP6 models, Sci. Adv., 6, eaaz9549, https://doi.org/10.1126/sciadv.aaz9549, 2020.