

Reviewer 1

Comment

In the introduction (line 85) and the discussion (line 498), the authors remark on oxygen minimum zones. This seems to me unnecessary, and possibly confusing, as the study region is not in or near an oxygen minimum zone.

Answer

We agree these two sentences may have been misleading as the NWES is not an oxygen minimum zone. Here we wanted to point out how the processes that shape large scale oxygen minimum zones are similar in smaller scale low oxygen areas. We clarified this in the text.

Comment

At some point, the abbreviation in $O_{2,phy-ch}$ should be explained. The term 'physico-chemical' does not appear in the text (I think) in the revised manuscript.

Answer

We explained the abbreviation phy-ch, physico-chemical, in the methods.

Comment

Ensemble members should be labeled in Fig. 2

Answer

We added ensemble labels to fig. 2. (NB the figure is now moved to an appendix after reviewer 3 suggestion).

Comment

As a general comment, the prose throughout the text are often a bit wordy or conversational. This can make the point hard to follow at times. The text would benefit from some simplification of sentence structure where possible.

Answer

We carefully read the paper and took care to improve on the style and simplify convoluted constructions as much as possible, especially in the results section.

Reviewer3

We thank the reviewer for the useful comments. We implemented the suggested changes as detailed below.

In particular, we took care of improving the justification of our methodology, we simplified and added clarity to the results exposition, and we moved some information to a supplement.

Major points

Comment:

The structure of the paper (mostly the results section) could be improved. I would recommend starting with the oxygen changes, which is the main focus of this study (i.e., move 3.3 to 3.1). Section 3.1 (Validation) could be relegated to an appendix or to Section 4 to discuss the reliability of the results. Section 3.2 (ΔT and ΔS) could be placed later when these variables are invoked to explain different mechanisms and correlations. Section 3.4 (contributions to O_2 change) seems that it should include 3.5 (contributions from T and circulation) and 3.6 (contributions from biology). The following Section (3.7; Impact of abrupt changes in circulation) seems a little off-beat given that it is the only section supported by a time series (Fig. 10). Maybe the conclusions from Fig. 10 can be presented also in Δ 's that match all the previous results/figures?

Answer:

We thank the reviewer for their suggestions to streamline the results section of the paper. As suggested, we moved the validation in the supplementary information. We still prefer to keep the order of the remaining sections as we originally envisaged, as this would allow to provide first a short summary of the impact of climate change on the NEWS T and S to provide the context in which oxygen changes will be analysed, and then continue to focus on oxygen for the rest of the section without any disruption. Similarly, we decide against accepting the suggesting in merging sub-sections 3.4, 3.5 and 3.6 (now 3.3, 3.4, and 3.5) as we believe that the current structure with sub-heading help the reader to navigate across the various aspect of the analysis.

We have added a sentence to highlight the rational at the beginning of the section.

Finally, regarding sub-section 3.7 (now 3.6), while we understand that being the only section where time series are presented might have initially puzzled the reviewer, we believe that showing maps of D of the circulation would be less informative than comparing the time evolution of the current with that of the oxygen change because the synchronous aspect of the changes would be missed.

However, we have changed figure 10 (now figure 9) showing D $O_{2,sat}$ and D SS instead of $O_{2,sat}$ and SS to be consistent with the other sections.

Comment

I remain unconvinced that the decomposition of the authors of $O_2 = O_2^{sat} \times SS$ is more useful than the traditional $O_2 = O_2^{sat} - AOU$. In the revised manuscript and to the other referees pointing to this in their first review, the authors responded that their method is different in that it focuses on change with respect to a reference period. However, this is entirely doable with AOU as well, simply through $\Delta O_2 = \Delta O_2 - \Delta AOU$. It thus appears that the SS decomposition only makes the paper unnecessarily convoluted.

Answer

We understand how our ΔO_2 decomposition needs additional support as also the other reviewers raised concerns about it. To answer these concerns we wrote down the equations to explicitly compare our ΔO_2 decomposition to the one the reviewer suggests, $\Delta O_2 = \Delta O_{2,sat} - \Delta AOU$. The full proof is in the supplementary material. In short, it can be demonstrated that, while the two decompositions are equivalent, ΔAOU does depend on both $\Delta O_{2,sat}$ and ΔSS (Eq S6).

This means ΔAOU doesn't just account for O_2 changes due to transport and biology, but also for part of the changes due to changes in solubility. This is problematic as AOU is usually interpreted as

the component of O₂ change not due to changes in solubility. We then go on to prove that this discrepancy is reconciled if we assume complete saturation at the initial conditions ($SS_{t0} = 1$), which, as the reviewer pointed out, is an assumption of the AOU model. This assumption is not present in our decomposition, and this allow us to better separate the effect of changes in solubility from the one due to the other changes.

we thank the reviewer for suggesting to be more detailed in the description of the mathematical passages behind our assumption and analysis as we believe these could be helpful for readers seeking to make sense of some common practice of O₂ change decomposition, that is given for granted in many studies.

Comment

To reduce the number of panels, shorten the paper, and clarify which features/mechanisms are robust across models, maybe the authors could merge some panels, as is commonly done in CMIP studies? For example, Figure 4 in Busecke et al. (2022; doi: 10.1029/2021AV000470) uses dots to indicate where most of the models disagree on the sign of the 2000–2100 O₂ trend in the Pacific OMZ. In a similar vein, to lend a helping hand to the reader, maybe the authors could use a distinct overlay/hash to indicate where they think the correlations are not to be understood as causal. Overall I think the paper would benefit from summarizing the Figures visually.

Answer

We thank the reviewer for the suggestion about hatching areas of interest, and we have adopted that in figure 3 to highlight the hotspots of de-oxygenation, that we defined with rigorous thresholds. Unfortunately, it is more difficult for us to define a clear threshold for the correlation to be considered not causal, because that assessment is done not purely on a quantitative metric (the strength of the correlation) but also on the existence of a causal relationship. To help the reader, we always specify the name of the areas using the same toponym used in figure 1, so that readers not familiar with the area could still orient themselves.

Furthermore, we're not sure that merging the panels for each model in a single panel similarly to Busecke et al (2022) would be beneficial in this case. In that paper the authors analysed a much wider ensemble (14 models) and so they needed to give an indication of how spread the ensemble was. In our case, we only have 3 members, and therefore the ensemble is too small to make any such consideration. Besides, looking at each model separately allow to better understand the causal relationship behind the observed changes.

We however recognise the result section may have been at times hard to follow. We made several changes throughout the section to alleviate this. We carefully modified the text, simplified the exposition whenever possible and avoided unnecessary repetitions. We put extra care on highlighting the information we want to focus on, especially causal relations, and we added references to panel labels to guide the reader.

Comment

Given that the authors focus on near-bed oxygen and thus benthic ecosystems, it might be good to consider changes in pO₂ rather than O₂ concentrations (as advocated by, e.g., Seibel (2011; doi:10.1242/jeb.049171) and Hofmann et al. (2011; doi:10.1016/j.dsr.2011.09.004)). Better yet might be to consider some metabolic index, e.g., such as the one by Deutsch et al. (2015; already cited by the authors), although that might arguably be out of the scope of this work. Importantly however, the authors should discuss the temperature dependence of the tolerance of benthic organisms to reduced O₂ (e.g., Deutsch et al., 2020; doi:10.1038/s41586-020-2721-y), which might exacerbate the impact of deoxygenation on benthic ecosystems.

Answer

We agree pO₂ or metabolic indexes (which are based on pO₂ rather than oxygen concentration) like the ones in Deutsch et al. 2020 or Clarke et al. 2021, are more meaningful for impacts on organisms than oxygen concentration. As the reviewer points out, our paper focusses on deoxygenation rather

than on its impacts. We however agree this an important point to discuss and we added a dedicated paragraph at the very end of the discussion.

Minor points

Comment

"ecosystem" can be replaced by "biological" in many places for clarity.

Answer

We replaced "ecosystem" with "biological" where it was fit.

Comment

Many long multiple-idea sentences could be split up.

Answer

We carefully re-read the text, taking care of splitting up long sentences with full stops whenever possible.

Comment

Avoid switching between "variables" and "parameters" if possible.

Answer

We made sure that in the text "variables" always refers to model state or diagnostic variables and "parameters" always refers to model parameters.

Comment

Avoid the use of "common to X and Y" and instead maybe use "the same in X and Y"

Answer

We replaced all "common to" with "the same in".

Comment

"Changes in ΔX " is incorrect. It's either just " ΔX " or "changes in X".

Answer

We made sure not to use "change in ΔX ".

Comment

What exactly is the correlation shown in most figures? Over what is it computed? Over the time periods? Both other referees requested equations in the previous review but only some quite unclear text was added.

Answer

The Spearman correlations are computed at each grid point between pairs of monthly averaged timeseries (so over time) for the whole time period. We modified the relevant text in the methods section to make this clearer. We don't feel we need to add an equation for Spearman correlation, which is widely used.

Comment

Minus signs should be proper minus signs "-" if possible (instead of hyphens "-")

Answer

We replaced hyphens with minus signs where fit.

Comment

Sentences starting without a capital letter should be fixed. Random capitals mid-sentence should be fixed. Typos persist in this revised version.

Answer

We carefully re-read all the text and corrected typos and capitalisations.

1. Introduction

Comment

L65: What are example of sub-lethal effects? I think one could be vision loss (e.g., McCormick et al. (2017; doi:10.1098/rsta.2016.0322)) but maybe the authors had other effects in mind that they should explicitly list here.

Answer

We were thinking more generally about depression of metabolism with low oxygen levels, but also impaired vision is a good and more specific example. We added a reference for each.

Comment

L70-74: Simplify to 2 significant digits and use the same unit (all in % or all in concentration) for clarity.

Answer

We agree it would be advisable to provide this information with coherent units. Unfortunately the Bopp and Kwiatkowski papers provide this information with different units and it is not possible to convert one in the other based only on the final papers. The aim of this sentence is to set up the context of declining ocean oxygen and comparing different global estimates is outside the scope of this paper.

2. Methods

Comment

Fig. 1: Add circulation arrows to guide the reader through the region dynamics if possible.

Answer

We added circulation arrows to Fig. 1 and indicated the western Norwegian trench current in the caption.

Comment

L180: Explain what climate sensitivity means: After how many years of $2\times p\text{CO}_2$ is the change in T given?

Answer

The equilibrium climate sensitivity is estimated once a model run reaches stationary conditions (hence 'equilibrium') after an instantaneous doubling of atmospheric $p\text{CO}_2$. This may take different time in different models. We don't think a quantification of this is necessary to follow the rest of the paper.

Comment

L190: Explain what the version differences mean. What has changed between them?

Answer

Changes between NEMO versions typically include numerical schemes, the representation of physical processes, modules coupling, software and hardware related matters, domain

configurations, etc. All of these are quite complex topics and it is out of the scope of this study to thoroughly describe them. It wouldn't be useful either because it is not straightforward to attribute any of our results to changes in NEMO versions; and indeed we don't. We think here it suffices to refer to the relevant literature where these are described with more detail.

Comment

L191: Explain what do the functional types difference applies to and what these differences are.

Answer

Here a similar reasoning applies. The two ERSEM versions differ in many regards. Noticeably in Phytoplankton and Bacteria parameterisation, although this is not the only thing that changed. These changes probably do have some effect on our results (which we do mention in the discussion). However, attributing a certain result to the different ERSEM version is not straightforward and would require ad-hoc experiments, which we don't have time or resources to run. And we don't think that would add anything key to our results either. We think it should suffice here to cite the relevant literature where the different ERSEM versions are described in detail.

Comment

L206: the ocean color data product needs a reference.

Answer

We added a reference for the Ocean Colour product.

Comment

L209: What does "setting low parameter values" do? Which parameters?

Answer

By setting biogeochemical model state variables to low values we mean positive but close enough to zero to be negligible (e.g. $1e-5$). This is because most model biogeochemical state variables cannot be negative or =0. We replaced "low" with "close to zero" in the text.

Comment

L209: What is "climatological" used for here? I think the authors mean "forced by climatological mean observations". Models can be deemed climatological too.

Answer

We replaced "climatological" with "forced with climatological mean values". Note that while Baltic boundary biogeochemistry is from observations, the physics are from a reanalysis product. This is specified in the following sentence.

Comment

L212: Space after dot is missing.

Answer

A space was added after the dot.

Comment

L216: That the nitrogen deposition field was "downloaded 2011" is not useful. Give a reference instead.

Answer

We added a reference for the N deposition product.

Comment

L216: Anything special or descriptive can be said about tidal forcing? Why two citations and no explanation?

Answer

Tidal mixing is recognised as an important process in the NWES and tides are indeed routinely applied in all recent NWES regional ocean model runs we are aware of. However, we think a comprehensive description of tides implementation would not just be lengthy, but also out of the scope of this paper because it wouldn't add anything substantial to the interpretation of our results.

Comment

L219: Is the "zero-gradient scheme" what is commonly known as Dirichlet boundary condition? If so name it that way.

Answer

The boundary scheme we use is not the Dirichlet one but a special case of the Neumann one where the gradient between each boundary cell and the adjacent interior cell is zero (hence zero-gradient). We think the sentence "the concentration at the boundary equals the concentration immediately inside the domain" should clarify this for readers.

Comment

L323–239: Rewrite nbias and nurmsd paragraph, which is currently obscure and repetitive. An equation for each term would not hurt, as suggested by the other referees before. Using equations and less text can be good for clarity and brevity.

Answer

We re-wrote the validation paragraphs (both the methods and results), also adding equations, and moved them to supplementary material, as suggested.

Comment

L244: The parenthetical is unclear: Enhanced stratification does not limit atmospheric oxygen uptake, at least not on the regional scale, and Changes in circulation include changes in lateral transport by definition.

Answer

Agree, we changed "atmospheric oxygen uptake" to "vertical transport" (i.e. from the mixed layer to deeper layers), but retained the "lateral transport" bit because it is indeed changes in ocean circulation that produce (also) changes in lateral oxygen transport.

Comment

L249+: What about "works as an approximation" instead of just "works". Also, what about "saturated" instead of "relaxed": AOU assumes complete saturation. Assumptions about it are not "change a little" but they are "does not change" instead.

Answer

Agree, we implemented the suggested changes.

Comment

Eqs. (1) and (2) are not useful in my opinion. Add an Equation for $O_2 = SS \times O_{2,sat}$ instead, if you must. Related: Maybe I missed it, but how are O_2 and SS computed? Is $O_{2,sat}$ an explicit tracer in the models? Is it computed directly from atmospheric pO_2 and in situ T and S ?

Answer

We removed equations (1) and (2). $O_{2,sat}$ is computed (at runtime by ERSEM) from temperature and salinity according to Weiss 1970 (atmospheric pO_2 is constant), $SS = O_2 / O_{2,sat}$. We added these informations in the text.

Comment

L269: The "discrete product rule" is not really a thing, although I guess it could be. (This is my fault for naming it that way, thinking it made sense as a comment. The "product rule" is a thing, but that's

not what the authors are using). Either way, this is basic calculus that does not need a name, so what about simply: "Oxygen change between t_0 and t can be decomposed as follows:"

Answer

We removed "by the discrete product rule".

Comment

L274: replace "being SS_t " with " SS_t being"

Answer

We agree, done.

Comment

Remove Eqs. (4) to (6), and add braces below Eq. (3) terms instead.

Answer

We think that adding braces below Eq. (1) terms may end up not looking too good on a typical 2-column paper layout. We would prefer to keep eq 2, 3 and 4 separated.

Comment

L297+: This false-positives part is a little obscure to me. Can the authors simplify it?

Answer

We re-wrote the paragraph, we think it is much clearer now.

Comment

L305: Replace " O_2 / AOU " with " $O_2 = O_2 - AOU$ " to avoid confusion. ("/" can mean "divided by")

Answer

We rephrased the decomposition as $O_2 = O_{2,sat} - AOU$

Comment

L306: The difference with AOU is not "the reference period". See major point.

Answer

As mentioned earlier, we added in the supplementary material a proof of how our decomposition differs from an AOU-based one. The difference is that our decomposition does not assume complete saturation at t_0 . We added this information in the text.

Comment

Make it clear here that Δ solubility captures most of the change in O_2 on the shelf because here intense vertical mixing dominates open ocean contributions.

Answer

We added a mention to the fact that vertical mixing dominates open ocean exchange.

3. Results

3.1 Ensemble Validation

Comment

So what? What is over/underestimated?

Answer

As mentioned, we re-wrote the validation sections and moved them to supplementary material, as requested. Among other changes we took care of explicitly stating what is over- and under-estimated in our ensemble members.

Comment

Delegate to appendix or discussion.

Answer

We moved validation to the supplementary material.

Fig. 2:

Comment

Colors would be welcome.

Answer

We had some trials adding colours to the validation figure. Unfortunately this doesn't help and actually makes things worse when symbols overlap, making them very hard to tell apart. We think however that the figure is still effective in summarising the validation results. Even when symbols are clustered, that indicates there's not much significant differences among them.

Comment

Add what is optimal/best in the caption. Is it (1,0), (0,0), or something else?

Answer

a perfect fit would be (0,0), i.e. 0 bias and 0 rmsd. We added this information in the caption.

Comment

Use words and function names in parentheses in the caption.

Answer

Unfortunately we don't understand what words and function names should go in parentheses. As we don't use parentheses for function names anywhere in the text, we don't understand why we should do it here. We are sure there's a point here. Unfortunately we struggle to understand it. We are happy to follow the reviewer suggestion once clarified.

Comment

Row labels are missing (I am guessing they are the 3 models).

Answer

We added the row labels

Comment

Maybe bad suggestion: since these are normalized metrics, the axes could be shared and only be shown on the left for the y-axis of the left-most panels and the bottom for the x-axis of the bottom-most panels (and the "cross" at (0,0) could be shown without the values for tick labels).

Answer

As the reviewer anticipates, using the same axes limits for all plots here is not advisable as it would further cluster together the sub-basin labels in most plots, making them unreadable.

2. Changes in temperature and salinity

Fig. 3

Comment

(Also applies to most maps) permuting the layout would allow for bigger panels and avoid requiring the reader to zoom in.

Answer

Please note that the figure size in the manuscript are not the actual final figure sizes. These will ultimately be up to the editors of the journal, however we sized our figures so that a 2-column 3-rows figure like Fig 2 has the width of one column in a typical 2-column paper layout (about 8.1 cm). With this figure size and with the appropriate resolution all panels will be clearly readable in the published paper.

Comment

(Also applies to most maps) units could be better placed near the colorbars rather than in the title.

Answer

Given how we structured the figures' layout, placing the units near the colorbar would come at the cost of having larger figures, without noticeable benefits for readability.

Comment

(Also applies to most maps) Discrete colormaps and filled contours could help for humans to extract values and visualize fronts.

Answer

We replaced all maps (except the bathymetry in Fig. 1) with filled contours and discrete colormap ones.

Comment

Show past and future T and S too in appendix/supplement?

Answer

Changes in physics variables T and S is what ultimately drives changes in stratification, circulation and biogeochemistry, including oxygen. We think T and S are an important feature to show in the main text.

3. Near-bed oxygen current state and change

Fig. 4

Comment

Show future O₂?

Answer

Here we deemed more appropriate to show ΔO_2 , rather than future O₂, because the Δ is not affected by model bias.

Comment

What are the red spots when zooming in?

Answer

The red spots were an artefacts from the bias correction due to the regridding of the observation data. We removed them.

Comment

Do the high hypoxia incidence coincide with the highest past O₂ levels? Is this meaningful to discuss?

Answer

This is indeed an important point to underline. In short, these areas have high productivity (high O₂ production) and stratify seasonally (hence vulnerable to hypoxia). The study focusses on changes in average values rather than extremes, so we didn't include a full diagnosis of this. However, we added some lines in the results and in the discussion.

Comment

Is there no hotspot O₂ decline for GFDL? What thresholds define hotspots?

Answer

We added a definition of hotspot for oxygen decline as areas with $\Delta O_2 < -0.5 \text{ mg L}^{-1}$ and $\Delta O_2 < 1.5 \Delta O_{2,\text{mean}}$, with $\Delta O_{2,\text{mean}}$ the average ΔO_2 over the shelf. This information was included also in Fig. 3 where hotspots of O₂ decline are now hatched.

4. Contribution to near-bed O₂**Comment**

L364: What is "negligible"? 10%? 1%? Less? It is important to be precise and quantify these terms because they are nonlinear (sometimes quadratic or worse), such that if they start gaining momentum as the climate changes, there is a chance they become dominant eventually.

Answer

$\Delta O_{2,\text{mix}}$ in the last 30y of simulation is on average $< 1\%$ of total change in all members. We added this information in the text.

Comment

L376: ΔSS_{O_2} notation unused elsewhere.

Answer

We corrected the typo.

Comment

Fig. 5: Colorbar tick labels of last column are rounded too aggressively.

Answer

We corrected the colorbar tick labels.

5. Physical controls of ΔO_2 : temperature and stratification**Comment**

L382: "Changes in $\Delta O_{\text{phy-ch}}$ " does not work.

Answer

Changed to "Changes in $O_{2,\text{phy-ch}}$ ".

Comment

Fig. 6: Why does the white turn gray for this figure?

Answer

Grey areas on shelf are where the correlation is non-significant. We added this information in the captions.

Comment

L397: missing punctuation before "de Boer"

Answer

We added a comma.

Comment

L401: Too many "is" in the sentence.

Answer

We removed "is" before "surface salinity".

Comment

L404: Replace "mediated" by "caused"

Answer

We replaced "mediated" with "caused".

6. Biogeochemical controls of oxygen change: primary production and respiration

Comment

L414: Not "all models": Δ NPP looks to be positive for HADGEM (more strong red).

Answer

Agree, we added this to the results description.

7. Impact of abrupt changes in circulation on the emergence of deoxygenation hotspots

Comment

L452: What are "R" and "p"?

Answer

We defined R and p in the methods. They are Spearman correlation coefficient (R) and p-value (p).

Comment

Fig. 10: panel labels are not consistent with previous figures, which have no ending parenthesis, e.g., "a" vs "a)".

Answer

We changed the panel labels.

4. Discussion

Comment

L465: Odd space

Answer

Odd space removed.

Comment

L466: Move Holt et al. reference to just before the comma.

Answer

Moved Holt et al. reference to after "changes" (because also Wakelin et al. reference is about the NWES).

Comment

L494: Remove "by critical hypoxia" (it is clear that you are talking about hypoxia for which you just defined the threshold)

Answer

Removed "by critical hypoxia".

Comment

L493: "Oschlies" is misspelled.

Answer

Corrected "Oschlies".

Comment

L533: Remove end of sentence: "testifying (...) in our ensemble" (redundant).

Answer

Removed "in our ensemble".

Comment

L536: Capitalize RCP and define it (and cite appropriate reference)

Answer

We capitalised RCP8.5. We would avoid, if possible, lengthy description of what Representative Concentration Pathways (RCP) and Shared Socioeconomic Pathways (SSP) scenarios are, and how they are defined, as this is common knowledge in climate science and it risks to unnecessarily weigh down the text.

5. Conclusions

Comment

L571: There is no "World" in CMIP.

Answer

Removed "World".