

egusphere-2023-1049-manuscript-version3 Review

Title: Multi-model comparison of trends and controls of near-bed oxygen concentration on the Northwest European Continental Shelf under climate change

This is my third review of this manuscript. In my previous review I recommended major revisions related to the paper's structure, the methodology, the figures, and other minor issues. The structure and the figures have been greatly improved, making the paper easier to follow and straighter to the point. The explanations are clearer and the conclusions better supported. Overall I think the manuscript now only needs minor adjustments before publication.

My biggest concern relates to the justification for the authors' approach for decomposing oxygen (they use $O_2 = SS \times O_{2,sat}$) and to its comparison with the "AOU approach" ($O_2 = O_{2,sat} - AOU$). In their last response and revision, the authors have added claims of advantages over the AOU approach, which are not supported, and some confusing/incorrect statements remain regarding the differences between the approaches and about the assumptions underlying the AOU approach. I emphasize that I am not suggesting the authors should change their methodology. It is entirely up to them to choose the approach they think is best suited to support their conclusions, even if that choice is arbitrary. However, unsupported claims and incorrect statements should be fixed or removed. Below I detail the issues relating to the authors approach, and then list some minor points and suggestions, which I think will require only minor revisions, hence my recommendation.

Authors approach vs AOU approach

1. In their response, the authors suggest that an advantage of their approach is that it allows to cleanly separate the solubility-only contribution while the AOU approach does not because ΔAOU contains a $\Delta O_{2,sat}$ term in (eq. S8):

$$\Delta AOU = (1 - SS_{t0}) \Delta O_{2,sat} - O_{2,sat,t0} \Delta SS.$$

There is a logical and a numerical issue with this argument:

1. The logical issue is that of circular reasoning, in that the argument is based on the implicit premise that ΔSS is independent of $\Delta O_{2,sat}$ in the first place. In fact, one could apply the same argument to ΔSS just as well. That is, one could write ΔSS as a function of ΔAOU and $\Delta O_{2,sat}$ through the Taylor expansion of $SS = 1 - AOU / O_{2,sat}$, and then argue that ΔSS contains $\Delta O_{2,sat}$ terms.
2. The numerical issue is that even if the premise (that ΔSS is independent of $\Delta O_{2,sat}$) were true, the detailed proof in the supplement is not (yet) supported by the data. In the proof, the authors argue that the $\Delta O_{2,sat}$ dependency would only disappear if $SS_{t0} = 1$ and that this condition is not verified in their case. However, $1 - SS_{t0}$ need not be zero. It just needs to be small enough (maybe $SS_{t0} \approx 90\%$ suffices). If so, the $(1 - SS_{t0}) \Delta O_{2,sat}$ term would be of second order and could be discarded just as the "mix" term. Figures showing the magnitude of SS (past and future values, not the change ΔSS) may help prove/disprove this point.

I would recommend removing these claims and arguments entirely, unless the authors can provide a proof that ΔSS is independent of $\Delta O_{2,sat}$.

2. The difference between the authors' approach and the AOU approach is incorrectly stated in the main text, where the authors have essentially kept the original statement suggesting that the difference lies within "accounting for a reference period", which I had already pointed as incorrect in my previous review. The reference period is separate and is not the difference between the two approaches. In fact, the authors themselves, in the supplement, use the same reference period for both approaches. I would thus suggest to not mention the "reference period" when discussing methodological differences with the AOU approach.
3. There is a recurring confusion between the condition of complete surface saturation ($SS_t = 1$ for all t at the surface, the assumption for AOU) with the condition of complete saturation initially everywhere ($SS_{t0} = 1$, which comes from the circular argument; point 1. above). This recurring confusion appears in both words and equations in the authors' response (to Reviewer 3, 2nd review, Major comment 2), in the main text (L270), and in the supplement (L102–103). This is strange because the assumption for AOU (surface saturation) is correctly stated elsewhere in the main text (L237) and even right after the incorrect assumption in the supplement, where the authors further suggest that they are the same (supplement L103–104):

(...) under the assumption that $SS_{t0} = 1$. This is in line with Duteil et al. (2013) that assumed saturation was reached at surface.

I would suggest removing any mention of this odd "initial saturation ($SS_{t0} = 1$)" condition which is not relevant to AOU.

Minor points/suggestions:

- L78:

whilst away from the euphotic zone respiration exceeds primary production, resulting in net oxygen consumption.

Is there **any** primary production away from the euphotic zone? If not, what about "whilst away from the euphotic zone respiration removes oxygen."

- L83: Replace "limit" with "reduce" for clarity
- L270:

The metrics in eq. 2 and 3 are related to the classic $O_2 = O_{2,sat} - AOU$ decomposition, with the difference that, by explicitly accounting for a reference period, they relax the AOU assumption of complete saturation at t_0 .

This is incorrect on two fronts: the difference does not lie in the reference time t_0 , and the AOU assumption is not complete saturation at t_0 (see major point above).

- Eq. (4): Note that "mix" as a subscript for the cross term could be confused for the contribution from (water) mixing.

- L274: Add comma

To assess what drives the oxygen changes we computed (...)

like so

To assess what drives the oxygen changes, we computed (...)

otherwise it may read as "the oxygen changes (that) we computed".

- L296: use minus signs instead of hyphens "-0.5 mg L⁻¹" (also in the exponent)
- L302: missing minus sign in exponent in "6 mg L⁻¹"
- L327: missing space after "Fig 3d)"
- L331: extra closing parenthesis in "(Fig. 4g-l))"
- L333: missing "the" before "Central North Sea"
- L375: missing space after "(Fig. 6d, e, f)."
- Fig. 7 caption: Capitalize 1st letter of 2nd sentence.
- L408:

(...) the correlation between SS and BResp is rather weak and, at some locations, positive (instead of negative as would be expected, Fig. 8e, f). This is because simple point-to-point correlation over the full period, does not allow to capture seasonally heterogeneous process.

Another reason could be that the point-to-point (in both time and space) correlation cannot capture the effect on SS from the distant respiration that occurred in the past and at upstream locations from where the SS is computed.

- L450: replace comma after "etc" with dot

Previous points where feedback was requested or required

- Sorry for this unclear comment:

Use words and function names in parentheses in the caption.

I assumed "nbias" and "nurmsd" were function names but maybe these are just acronyms? Anyway, what I intended to suggest was to use, e.g.:

Fig. S1. Validation results. Plots show normalised bias (nbias) vs normalised unbiased root mean squared (nurmsd) for selected variables in the three ensemble members and in different model subdomains. A perfect fit would sit at the origin (0.0, 0.0).

- Fig. 2 (formerly Fig. 3) comment:

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

Sorry for not being explicit enough. I did not mean for the authors to move or remove this figure. Instead, I meant to suggest **adding** a supplement figure of past and future T and S to provide additional information and context to this ΔT and ΔS figure.

- Fig. 3 (formerly Fig. 4) comment:

Show future O_2 ?

As point above there I also wanted to suggest **adding** plots of future O_2 without removing ΔO_2 .