Rev3:
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.

Answer:
We thank the reviewer for the useful comments, we have implemented all the suggested changed. In particular we amended both the methods section and the supplement and we no longer claim advantages of our approach over AOU. We also amended and/or removed all passages that were critical regarding the differences of the to approaches. All changes are detailed in the following.

We appreciate that the reviewer has not suggested to change the methodology, that we believe has similar validity of the more traditional AOU approach. We accept the reviewer that the difference between the two approaches is smaller than we implied and therefore we have decided to remove the final part of the supplementary information (from line100 onwards) and add the following:
"In this region, the first term ((1 – SSt0) ΔO2,sat) is usually small at the annual scale (figure S2), and therefore the two approaches are largely equivalent."

Authors approach vs AOU approach.

Rev3:
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.

Answer:
The reviewer is right in pointing this out. ΔSS and $\Delta O_{2,sat}$ are indeed assumed to be independent. We added one sentence in the methods explicitly stating the assumption of independence. This can be justified in our case study with the same argument that justifies the choice of the method for our

case-study. The timescales at which O_2 equilibrates with atmospheric concentration are short (due to intense wind and tidally-driven mixing) and ocean-atmosphere exchange dominates over shelf-ocean exchange. This effectively decouples the dynamics of $O_{2,sat}$ and SS, making them, as a first approximation, independent.

Rev3:
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} 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 $\Delta O_{2,sat}$.

Answer:
The reviewer is indeed right in pointing this out. We have calculated the $(1 - SS_{t0})\ \Delta O_{2,sat}$ term and it is indeed much smaller than the $O_{2,sat,t0}\ \Delta SS$ term. We added the plot to the supplement (past and future SS plots are also in the supplement).

Rev3:
3. 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.

Answer:
We removed any reference to the “accounting for a reference period” from both the main text and the supplement. Given the numerical results from the previous point, we can conclude our approach and the one based on AOU are equivalent for practical purposes at the spatial and temporal scales considered in this study. We now only state this in the main text and supplement and we have removed the portion of text that states otherwise.

Rev3:
4. 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.

Answer:
Contextually with all the other changes, we have removed any reference to “complete saturation at t0” from both the main text and the supplement.

Minor points/suggestions

Rev3:
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."

Answer:
We implemented the suggested change

Rev3:
L83: Replace "limit" with "reduce" for clarity

Answer:
We replaced “limit” with “reduce”

Rev3:
L270:
The metrics in eq. 2 and 3 are related to the classic O2 = $O_{2,sat}$ − AOU decomposition, with the difference that, by explicitly accounting for a reference period, they relax the AOU assumption of complete saturation at t0.
This is incorrect on two fronts: the difference does not lie in the reference time t0, and the AOU assumption is not complete saturation at t0 (see major point above).

Answer:
We removed here and from the rest of the text any reference to “complete saturation at t0” and “reference time” (see answer to major point).

Rev3:
Eq. (4): Note that "mix" as a subscript for the cross term could be confused for the contribution from (water) mixing.

Answer:
we replaced the subscript “mix” with “sord” (for Second ORDer).

Rev3:
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”.

Answer:
We added the commas.

Rev3:
L296: use minus signs instead of hyphens "−0.5 mg L^{-1}" (also in the exponent)

Answer:
done.

Rev3:
L302: missing minus sign in exponent in "6 mg L^{-1}

Answer:
done.

Rev3:
L327: missing space after "Fig 3d)"

Answer:
done.

Rev3:
L331: extra closing parenthesis in "(Fig. 4G-l))"

Answer:
done.

Rev3:
L333: missing "the" before "Central North Sea"

Answer:
done.

Rev3:
L375: missing space after "(Fig. 6d, e, f)."

Answer:
done.

Rev3:
Fig. 7 caption: Capitalize 1st letter of 2nd sentence.

Answer:
done

Rev3:
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 occured in the past and at upstream locations from where the SS is computed.

Answer:
Our method may indeed fail when causes and effects are spatially decoupled, which we did discuss (L522-526). We don't believe this is the case here though: in IPSL and GFDL BResp decreases everywhere north of (and including in) the Central North Sea (Fig. 8 b,c). Due to the counterclockwise circulation any signal from distant respiration (or in this case lack thereof) in the Central North Sea would originate along the East coast of Britain, Shetland and Irish Shelf, where also BResp decreases. In the same regions SS does mostly increase, albeit little (Fig. 4 k,l). Hence we would still expect a negative correlation even if the signal came from upstream.
On the other hand we do present evidence that during winter months, when respiration is dominant over primary production, the correlation is negative as expected (Fig 8 h,i).

Rev3:
L450: replace comma after "etc" with dot

Answer:
done.

Previous points where feedback was requested or required

Rev3:
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).

Answer:
We modified the caption as suggested.

Rev3:
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.

Answer:
We added figures to the supplement showing present and future temperature and ssalinity.

Rev3:
Fig. 3 (formerly Fig. 4) comment:
Show future O2?
As point above there I also wanted to suggest adding plots of future O2 without removing ΔO2.

Answer:
We added figures in the supplementary material showing present and future O_2, $O_{2,sat}$ and SS.