

egusphere-2026-304: Author response to review 2

Koeve & Frenger: A case for a pragmatic oxygen-based approach to quantifying the biological contribution to the marine carbon sink

Dear editor & reviewers,

We thank both reviewers for their time and very helpful and constructive comments. Please find below point-by-point responses to the comments. Original reviewer comments are given in **black**, our responses in **blue**.

We also provide modified text elements, where it is straightforward (in **red**) and indicate where additional text changes are planned for the revised version.

Reviewer 2

The authors revisit the concept of “Apparent Oxygen Utilization (AOU)” in this manuscript. They assess the usefulness of the AOU approximation for the preindustrial steady state and for decadal-to-centennial climate change projections using the UVic Earth System Model (ESM). Moreover, they have extensively evaluated the usefulness of AOU as an estimate of the soft-tissue carbon pump.

It has been widely discussed that uncertainties in AOU arise from air-sea disequilibrium and from the interpretation of AOU relative to “True Oxygen Utilization (TOU)”. The authors have designed a suite of sensitivity experiments to quantify uncertainties under various boundary conditions (i.e., scenarios). The AOU (and its extension to quantify the biological carbon pump) has been widely used for observational and model diagnostics (e.g., deoxygenation studies), and it is important to assess its uncertainties and interpretation. The authors introduced (revisited) the concept with a nice introduction, methods, and discussion based on sensitivity experiments. I think it is important to discuss the widely used AOU concept to better understand the ocean oxygen and carbon cycles. I enjoyed reading the paper, and the study will be a great contribution to advancing our understanding of ocean deoxygenation and carbon uptake under climate change.

A: We thank the reviewer for his supportive words.

I still have several comments for the paper, and I hope this helps to improve the manuscript.

A: We thank the reviewer for his/her constructive comments and respond below, point-by-point. See our detailed responses below (in blue). Where obvious, we provide also planned changes to the text (in **(bold) red**).

Major Comments

R2.1. I see this study is focusing on further (or re-) interpreting the concept of “AOU”. I believe, in general, we think of AOU as an approximation of TOU (under the assumption of perfect equilibration at the surface with given T and S, as the authors also stated), but the

authors introduced further interpretation, including “disequilibrium” (more specifically, biologically driven disequilibrium) to interpret AOU from a different point of view.

I understand what the authors are trying to do in this paper, but this needs clarification in the main message (especially in the abstract and conclusion). Moreover, I am wondering whether the authors are arguing that AOU and TOU provide slightly different information (by including the impact of air-sea disequilibrium stemming from both physical and biological processes). I think there is room to revisit the presentation style to highlight this rather than just introducing the interpretation of the AOU and TOU. This relates to the minor/specific comment I made for L247, which I think needs clarification on the difference between AOU and TOU from the author's perspective (and whether the authors are arguing both useful concepts, but interpretation needs to be made with caution). Particularly, the meaning and difference between “... accumulated O₂ utilization of interior ocean waters since last contact with the atmosphere (for TOU)” and “the total accumulated O₂ debt associated with the degradation of organic matter in the interior ocean (which I think points to the AOU)” in the main text were not entirely clear to me (I usually think the former is the basic interpretation of both AOU and TOU).

A: We thank the reviewer for this suggestion. In the revised manuscript we point out early on that with our manuscript we stick to original definitions of TOU and AOU but provide insights into differences between the two. In particular we aim to push forward that the fact that AOU differs from TOU is not a weakness. Instead it contains information on organic matter decay prior to the most recent contact with the surface (but not lost due to equilibration). AOU is hence more a comprehensive measure of the oxygen missing and carbon added due to organic matter degradation.

We will add a summarizing paragraph to the conclusion and also make changes to the abstract, accordingly.

To provide background/history: The first, though brief and rather theoretical, treatment of the term “true oxygen utilization”, TOU, that we could track down was in the Broecker and Peng 1982 textbook (Tracers in the Sea, p 131). They interestingly concluded that the assumption of oxygen saturation is likely an underestimate of preformed oxygen leaving the surface ocean, hence AOU to be an underestimate of the true oxygen utilization. This was based on the available data at the time (in particular summer time GEOSECS observations), which showed a slight supersaturation (on average 3%) of surface waters compared with oxygen solubility computed from surface temperature and salinity, explained in particular by bubble entrainment (Fig. 3-6, a plot confirmed in later text books, e.g. Sarmiento & Gruber, 2006, from more recent and extensive data.) Broecker and Peng also mention the additional effect of mixing waters with different endmember temperatures on computed O₂^{sat}, but conclude that “these effects are small compared to actual utilization, AOU is quite close to TOU (true oxygen utilization)”. Ito et al., 2004 were the first to introduce an explicit model tracer approach to quantify TOU and the difference between AOU and TOU explicitly. Instead of using a tracer of TOU directly (as we do here), they compute TOU by difference between O₂ and a tracer of preformed oxygen which is set at the time of subduction and transported by physical circulation through the interior ocean. Ito et al. 2004 clearly show in their model experiments that waters in regions (and at times) of water mass subduction, in particular in the high latitudes, are often undersaturated. This undersaturation was found to be a

consequence of the upwelling or convective mixing of interior ocean waters low in oxygen due to former organic matter degradation. The fact that convective mixing is often combined with heat flux out of the ocean (i.e. surface water cooling and increase in oxygen saturation) adds to initial oxygen undersaturation.

Both Broecker and Peng as well as Ito et al. consider TOU as a property accumulating since last contact with the surface indeed. It is unfortunate (as, we find, misleading) that with coining it 'true' this property appears to reflect the 'true' oxygen utilization while it includes 'only' the degradation products since last contact with the surface/atmosphere'. As we show in this study, the products of organic matter degradation for oxygen and carbon can be recirculated when water subducts prior to equilibration.

R2.2. It is interesting to see how AOU based estimates of soft tissue carbon pump diverge from the DIC-based estimates. In climate change conditions, the authors explain this by the impact of the $C_{\text{anth-effect}}$ (L374-...), but I am still not sure why the transient increase in atmospheric $p\text{CO}_2$ impacts $\text{DIC}_{\text{dis, bio}}$. I understand the physical-chemical invasion of anthropogenic carbon could blind things, but I would like to ask for further explanation and clarification on why this impacts the $\text{DIC}_{\text{dis, bio}}$, not the $\text{DIC}_{\text{dic, phys}}$.

A: Though it is difficult to show this explicitly, we suspect "buffer erosion" as reason for the impact of the C_{anth} invasion on DIC^{dis} . Specifically, it is the dependence of the equilibration time of CO_2 (but not oxygen) on the carbonate buffer factor (Broecker and Peng, 1974, Tellus 26,1-2; Zhou et al., 2025, <https://doi.org/10.1038/s41558-024-02179-9>).

We change our text as follows (L642ff)

"Instead, the change in $\text{DIC}^{\text{dis, bio}}$ in the BGC*-experiment is a response to the transient increase of atmospheric $p\text{CO}_2$ (Fig. S8b), hence rather related to the physical-chemical invasion of C^{anth} . This C^{anth} -effect in BGC* experiment on $\Delta\text{DIC}^{\text{dis, bio}}$ can be explained by the impact of the carbonate buffer factor on the equilibration time of CO_2 , τ_{CO_2} , which can be estimated as $\tau_{\text{CO}_2} = \beta z_{\text{ml}}/k$, with $\beta = \partial[\text{DIC}]/\partial[\text{CO}_2]$, the carbonate buffer factor, z_{ml} the mixed layer depth and k the gas transfer velocity (e.g. Zhou et al., 2025). The carbonate buffer factor decreases with the invasion of C^{anth} into the ocean, hence does τ_{CO_2} . Since oxygen does not have a respective (O_2^{anth}) effect ($\tau_{\text{O}_2} = z_{\text{ml}}/k$), an AOU based estimate of C^{soft} is blind against the C^{anth} -effect."

In addition, when authors conducted no biological pump experiments, did you also turn off the carbonate pump (calcium carbonate formation)? (I see the statement turning off the impact of biological carbon pumps on alkalinity and DIC is disabled for nO2bioPumps). I understand the AOU (C_{soft}) mainly focuses on soft-tissue pumping, so it is a separate topic, but I am wondering how this could affect the carbon in your suite of sensitivity experiments, especially the carbon disequilibrium, which may impact the results.

A: Yes we did also turn off the CaCO_3 counter pump. This is done for a simple technical reason. In our noBioPump experiments, we turn off the soft tissue pump by setting the PO_4 nutrient field to zero in the restart file of the noBioPumps spinup experiments. This stops primary production right away and causes over the spinup the loss of all remineralised DIC (and the invasion of oxygen into our model ocean. Since the CaCO_3 -pump depends prognostically on the soft tissue pump production in our model (Keller et al., 2012), the

former is disabled as well by definition. Running a model with CaCO_3 -counter pump but without soft tissue pump is likely difficult or impossible in most ocean models. We clarify in the Methods as follows (L269f):

“The setup noBioPumps is realized by setting PO_4 to zero globally, which consequently turns off both biological carbon pumps, the soft tissue pump and the CaCO_3 counter pump. “

R2.3. Finally, it would be helpful to see statements on how we can use the concept of AOU (and TOU) for future model diagnostics, such as CMIP model diagnostics, given the diverse treatment of ocean biogeochemical components in the model and differences in circulation and mixing. This is somewhat general, but it will still be nice to see the author's opinion and comments on facilitating the future analysis for better understanding of the ocean deoxygenation and changes in the carbon cycle.

A: The intention of our work is to provide quantitative bounds on the uncertainty of the AOU-approximation, not necessarily to give advice to Earth System modellers on how to extend their models. **However, we will add a short respective paragraph to the conclusions.**

Minor/Specific Comments

R2.4 How could the difference in air-sea gas exchange and equilibrium timescales between O_2 and CO_2 affect the results, given that the authors highlight the disequilibrium argument in this study?

A: See our response to R2.2 and the associated text addition.

L42: “Wilson et al., 2022” font should be fixed (from italic to non-italic).

A: done

L57: I think this is a good point; the assumption is not only from the perfect surface O_2 saturation but also stems from mixing and subsurface warming. Could you also clarify how subsurface warming could occur (are you thinking of shortwave radiation penetrating beyond the surface)?

A: Yes, in addition to mixing, penetration of shortwave radiation beyond the surface mixed layer was the point put forward by Dietze and Oschlies. We clarify (L 68):

“and subsurface warming (**penetration of short wave radiation beyond the surface mixed layer**) affect local temperature”

L122: How does wind speed, sea state, sea ice cover, etc., affect the biological part of disequilibrium? ($\text{O}_{2,\text{dis, bio}}$)

A: As with any disequilibrium, larger wind speed may reduce it and sea ice cover may enhance it. With this statement we wanted to clarify that the term $O_2^{\text{dis,phys}}$ refers particularly to the physical 'drivers' cooling or warming (eventually also salinity changes) for an oxygen disequilibrium. Our respective terminology is consistent with the one used in glacial-interglacial study by Cliff et al., 2021, <https://doi.org/10.1038/s41561-020-00667-z>. Compare paragraph starting in L 191 ("Since O_2^{abiot} has no biological ..."). No changes of text.

L198: How much of a difference does it make to run CO₂-emission-based simulations (compared to the prescribed CO₂ simulations as in Arora et al., 2020)

A: Interesting questions (whether the strength of climate-feedbacks depends on emissions vs. concentration forced). A direct comparison to Arora et al., 2020, however, is very difficult since we did not use the protocol of Arora et al., 2020 (1% pCO₂atm increase per year quadrupling of preindustrial values), but a scenario with much lower peak pCO₂^{atm} and warming. In Arora's 4xCO₂ simulations carbon climate feedbacks reduce the marine carbon sink by about -77 Pg C (Fig 3, CMIP6 models, model mean), compared to 670 Pg C in the BGC experiment, i.e. by about -11% on average. The intermodel-SD of the carbon-climate feedback parameter is 4.95 Pg C °C⁻¹ (Tab. A1, equivalent to about 30% of the mean carbon-climate feedback parameter). Adopting this SD-value to the numbers given above, I estimate that carbon climate feedbacks reduced the marine carbon sink by roughly -8 to -14% at 4xCO₂ in CMIP6 models. Our emission driven experiments show a cumulative marine carbon sink of 296 Pg C in COU* and 320 Pg C in BGC* at peak pCO₂^{atm} (year 82), i.e. a climate-change related reduction of about -7.5%. This appears to indicate a lower carbon-climate feedback. We note, however, that the strength of climate change (much larger in Arora's experiments) and also the time of comparison matter, as the impact of climate change continues over time will into net-zero emission states. In year 140 in our experiment (that is the year when 1%pCO₂ increase experiments reach 4xCO₂), the marine carbon sink of COU is by 10% lower compared to the marine carbon sink in BGC. I conclude that, unfortunately, we can't answer the question of the reviewer given the experiments at hand.

L205: "or the rate with which the sinking speed increases over depth", this should be clarified more by adding sinking "organic particle" speed.

A: added

L222: It is about the style, the sentence "Globally, most of this difference, about 80%, is explained - rather than by the, primarily, thermal effect of solubility increase due to cooling - by upwelling of undersaturated waters that carry the O₂ debt stemming from organic matter." is not easy to follow. I suggest rephrasing it. (I think the authors are arguing that most of the difference (about 80%) between AOU and TOU could be explained by upwelling of undersaturated waters ... correct?)

A: We have rephrased this section (see also Reviewer 1, comment C21) to:

“(L395) Globally, most of this difference, about 80%, is explained by upwelling of undersaturated waters that are low in O₂ due to organic matter degradation in the interior ocean (Fig. 1a). This ‘biological’ contribution is quantified by O₂^{dis,bio} (Fig. 1a, Fig. S2; Eq. 4). ... (L 320) The remaining 20% of the globally integrated O₂^{dis} is explained by the thermal effect of solubility increase due to cooling during the process of dense water formation and subduction (Fig. S2). “

L247: “(TOU) ... it is in fact not a good measure of the total accumulated O₂ debt associated with the degradation of organic matter in the interior ocean.”

This is an interesting point (and somewhat a bit confusing point to me as mentioned in the major comments), I usually think of AOU as an approximation of TOU, which means AOU also represents the accumulated O₂ utilization of interior ocean waters since last contact with the atmosphere (i.e. depends both on water mass pathways, age of water mass, and degradation of organic matter along the pathways), but the authors claim that AOU provides a good estimate of “total biological effect TOU+O₂^{dis,bio}”. Do authors think we should treat the AOU and TOU differently for interpreting the O₂ changes (rather than approximation of TOU)?

The authors also argue that under various circumstances (tested through a suite of sensitivity experiments), the uncertainties between AOU and “TOU + O₂^{dis,bio}” are ~ 10%, so I believe it is important to clearly state the reinterpretation of AOU in the manuscript (again, as mentioned in major comments).

A: We add respective text to the conclusions, see response to R2.2.

L258: “... in year 100”

Is this true? Looking at Fig. 2, the magnitude of Δ AOU and Δ TOU differs significantly by year 800 (we do see differences by year 100, but it is hard to see a 30% difference from the graph, perhaps because of the scale). It will be nice to include the actual numbers (magnitudes).

A: Will be improved in revised manuscript.

L298, 303-304: Minor point, but “ Δ AOU is robustly able to estimate Δ TOU+ Δ O₂^{dis,bio} to within about 10% uncertainty”, (under various model setups). Good to see low uncertainties across various scenarios, but where do you think this 10% difference stems from (numerics in the model, or other factors)?

A: We attribute the 10% difference to changes of O₂^{dis,phys}, i.e. a decrease of the magnitude of O₂^{dis,phys}. Like with the decrease of the magnitude of O₂^{dis,bio} (Fig. 2a), this decrease is likely explained by loss in sea ice (Fig. S3d), consistent with the findings of Ito et al., 2004, that the difference of TOU and AOU is strongly influenced by sea ice (in the Southern Ocean).

We amended the text as follows (L 516)

“The remaining (unexplained) change of AOU is attributed to a decrease of the magnitude of $O_2^{\text{dis,phys}}$. We suggest that the decrease in magnitude of $O_2^{\text{dis,phys}}$ and $O_2^{\text{dis,bio}}$ is associated with the sea ice loss (Fig. S3d) under global warming, as sea ice was found to be a prominent control of the difference between AOU and TOU in earlier studies (Ito et al., 2004; Duteil et al., 2013).

We will also prepare & add Figures of the change of $O_2^{\text{dis,phys}}$ in SI.

L298: “ $\Delta\text{AOU} \approx$ ” font should be fixed.

A: fixed

Fig. 3: I suggest including a legend explaining markers (circle, cross, etc.).

A: Will be added to the revised figures.

A: Again, we thank the anonymous reviewer 2 for her/his time and very helpful review.