egusphere-2026-304: Author response to review 1

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 1 (Benoît Pasquier)

Summary of the Manuscript

This short manuscript by Wolfgang Koeve and Ivy Frenger offers a valuable analysis of the pertinence and accuracy of apparent oxygen utilisation (AOU) for quantifying the biologic contribution to current and future oxygen levels in the ocean. For context, AOU ≡ O2sat(T, S) − O2 is an approximation of the accumulated oxygen consumption by bacteria as water transits in the ocean interior. AOU is thus an approximation of true oxygen utilisation (TOU), which is defined as the exact amount of oxygen consumed along the same transit. However, AOU has more practical utility than TOU because AOU only requires O_2, temperature, and salinity. As such, AOU can effectively be "observed" in situ, as opposed to TOU, which requires a model that precisely tracks O_2 consumption. But AOU also has caveats. In particular, its uncertainties are rarely quantified, so that cautious oceanographers rightfully worry about its accuracy and whether it can be used reliably (Ito et al., 2004). Nevertheless, the authors argue that part of these uncertainties are useful as they unintentionally capture biological effects that TOU does not, and thus that AOU is a better metric for quantifying the biological contributions to O_2 levels and future changes. The authors argue that the part of the uncertainty that is useful essentially comes from surface disequilibrium effects due to biology, and that this part generally dominates the uncertainty due to physical processes, which can hence be safely ignored. In summary, the authors argue that AOU can be safely used in more contexts than currently recognised, with broad implications for studying past and future ocean climates.

Overall Recommendation

This is a very nice and interesting study, that I hope will be promptly published in Biogeosciences, but I don't think it is ready yet. My overall recommendation is to send the manuscript back for revisions with the intent to publish once all issues are addressed. I must note that I hesitated marking these as "major" as they affect most of the manuscript, but it is quite short, so that in practice I think these revisions will not take that much work, so I ended up marking them as "minor".

While I understood (I think) the details of the paper, the process of understanding was greatly hindered by some choices made by the authors, and some missing pieces in the explanations. I list my Referee General Comments (GC) first and then my more detailed Referee Comments (C) below.

A: We thank Benoit Pasquier for a very detailed and helpful review. See our detailed responses below (in blue).

General Comments

GC1. "debt": I see a terminology issue here. Maybe there is precedent in the literature that I don't know about, but I find "O_2 debt" a little confusing here, and I feel like it does not fit. To me, "debt" means something that is owed or that must be repaid/rectified somehow. As far as I understand, there is an existing concept of "O_2 debt" used in biology to describe the oxygen deficiency in an organism (e.g., Taylor, 1982), but that does not fit here either. I strongly suggest avoiding this terminology of "debt" and to instead rely on the well-established, existing terminology that the authors already use anyway.

A: We agree with the reviewer that this term may be misunderstood, given its (primarily) physiological meaning (s.a.). We have changed the text respectively throughout the manuscript.

GC2. Biological contribution: I have carefully thought about decomposing drivers of oxygen changes before (Pasquier et al., 2024) and yet, on my first attempt, I became more and more confused about what the authors were trying to do with their comparisons between AOU and TOU as I read through the manuscript. The fact that some statements are simply incorrect about what AOU and TOU are did not help (see, e.g., **C27**, **C28**, **C29**).

A: Please see our responses to C27, C28, C29 below.

My main issue is that the authors did not clearly define what the oxygen's biological contribution is. It would be much clearer in my opinion to define it as the tracer O_2^{bio}:

$$O_2^{bio} \equiv O_2 - O_2^{abiot}$$

This definition (the "≡" symbol means this is a definition, not a mere statement of equality, although some prefer ":=") is clear and intuitive: The biological contribution is the residual between the base state and the abiotic state. This can be written as the natural decomposition, $O_2 = O_2^{bio} + O_2^{abiot}$. Then one can very simply show that

$$O_2^{bio} = (O_2^{pre} + TOU) - (O_2^{pre} - O_2^{dis,bio}) = O_2^{dis,bio} + TOU,$$

and proceed to compare AOU and TOU to O_2^{bio}. Or proceed to show that

$$AOU = O_2^{bio} + O_2^{dis,phy}$$

and proceed to show that $O_2^{dis,phy}$ is small, so AOU ≈ O_2^{bio} (Plotting $O_2^{dis,phy}$ would be helpful for this argument.)

A: We have carefully considered defining a quantity O_2^{bio} as suggested above. However, based on discussions with colleagues we find that defining TOU+$O_2^{dis,bio}$ as O_2^{bio} as proposed above causes a focus on the very upper ocean/euphotic zone oxygen produced during photosynthesis (i.e. biologically produced oxygen), and the implicit notion of O_2 oversaturation caused by marine biology. See frequently quoted statements that the ocean provides every second breath for humans (a misleading statement, see Gattuso et al., 2022, https://doi.org/10.64628/AAK.3cvnnevyf)). We expect such a perception to be further supported with a positive sign convention of AOU (see following reviewer comment and response, GC3). In this manuscript we focus on the interior ocean oxygen and carbon conditions that are dominated for oxygen by usage (not production) of O_2 by biology.

Hence we hesitate to follow the reviewers suggestion to introduce a term O_2^{bio}. However, **acknowledging the need to simplify our terminology for the *total biologically stripped oxygen* in the ocean interior, we introduce the term O_2^{util} for the sum of TOU+$O_2^{dis,bio}$** (new equ. 5 and related text, L 206ff of revised ms) and have changed the manuscript accordingly.

GC3. AOU/TOU sign convention: Going against the well-established sign convention is not only confusing but also technically incorrect. The term “oxygen utilisation” clearly means that it accounts for the amount of oxygen consumed, which can only be positive (there is no “negative consumption”). The author’s choice to switch the convention reads as absurdly as, e.g., “spending minus 10 euros for a meal” or “using −10 L of petrol to go there”. I **strongly** suggest the authors should reconsider this convention choice.

A: We argue with the following reasons to stick to the negative sign for AOU for our manuscript: Covering aspects of both the oxygen and carbon cycle, we need consistent reference points. Naturally a reference point for gas is the potential saturated state (O_2^{sat} and DIC^{sat}, respectively). Our definition of AOU follows the mathematical standard to subtract these reference points from the actual state. Also using a ‘positive definite’ AOU, would require to apply a positive conversion factor when computing the carbon equivalent of an oxygen metric of O_2^{util} (e.g. AOUx$R_{c:o2}$) which is biologically not meaningful. Since the degradation of organic matter increases DIC and decreases oxygen, we argue that metrics of organic carbon degradation products should have a positively defined carbon and a negatively defined oxygen metric. We finally note that historically the quantity AOU has been used originally to assess oxygen patterns in the ocean, with no explicit need to think about the sign. We consider O_2 and CO_2 in conjunction, forcing us to go for a consistent sign approach. We acknowledge that with this approach we deviate from the more conventional/traditionally sign definition of AOU (and TOU).

To clarify further, we rephrased in the text around Eq. 6, L 216-235.

GC4. Figures: Some figures require quite some energy to parse and understand. In the days of AI-assisted coding, LLMs can be used very efficiently to make publication quality plots, with colors, legends, text labels, and so on, in minutes. I have laid out many detailed comments below about them, and while some may be a matter of taste, they are grounded in fairly consensual principles in the science of making scientific figures. I think this manuscript would greatly benefit from better figures.

A: We agree that the figures benefit from improvements and will do so accordingly for the revised version of the manuscript.

Comments

C1. N cycle effect: What about denitrification? Is that cause for concern too as it could decorrelate ∂O2/∂t and ∂DIC/∂t?

A: Good point, which so far, we did not mention in our manuscript for the reason that pelagic denitrification contributes less than 1% to the globally integrated rate of interior ocean degradation of organic matter (in our model). However, denitrication affects DIC^{remin} but not TOU. Consistently, we find a very small difference between the change of DIC^{remin} and TOU (Fig. S7; ΔDICremin > ΔTOU). Respective systematic differences may be a matter of concern locally, e.g. in oxygen minimum zones. For example, an increase in denitrification would not be measured by a change in AOU (neither TOU), but contributes to increasing DIC^{remin}.

C2. “confronting the model” is used throughout instead of just “running the model”. This is a matter of style I guess, but it appears a little awkward for me.

A: We changed to “running the model”.

C3. No space between unit and element, e.g., “mmolC / m3”, not “mmol C / m3” otherwise it parses as “mmol times C per m3” instead of “mmol of carbon per m3”. This applies to the entire manuscript, including figures and tables. Please check carefully.

A: We will follow the suggestion of the Reviewer. We also carefully checked our manuscript for outdated usage of SI-units (e.g. “ / m^3”) and changed to the correct form (e.g. “ m^{-3}”).

C4. Use “×” symbol for multiplication instead of “*”. This applies to the entire manuscript, including figures and tables. Please check carefully.

A: Changed in the main text accordingly.

C5. The use of “variant” is a little confusing when the authors real;y mean “model configuration”. Please clarify and be consistent.

A: We changed model variant to model configuration.

C6. Throughout, the authors use "A (B, C)" or "A and B (C)" to refer to "A, B, and C" or simply "A, B, C". These inconsistencies are a little distracting and confusing sometimes.

A: done, Methods, in L 99ff.

C7. The authors use the word "idealized tracers" for preformed and regenerated tracers, but this is incorrect : These tracers are not idealised, they are just tracers that cannot be observed and must be modelled. This is because models allow us to "tag" the preformed/regenerated property onto the tracer as it enters/leaves the surface. I suggest to just call them "preformed" and "regenerated" throughout.

A: done

C8. L14: "with the atmosphere" → "with the surface"

A: Done

C9. L24: quantity → metric

A: Done

C10. L25: given → calculated from

A: Done

C11. L25: concept → assumption

A: Done

C12. L44: Why is it pragmatic?

A: The approach is pragmatic since it is, as indicated earlier in the ms (L 23-25) based on readily available information (T, S, O2). We rewrite to (L54):
"The contribution of the soft tissue pump is not directly measurable. Hence an indirect, 'proxy', approach is needed if we want to quantify this contribution to the marine carbon sink, both in models and future ocean observations. The AOU concept provides such an approach. It is pragmatic in that it is not perfect (comes with some uncertainty) but is better than nothing/is sth that we can use, as being based on readily available data (T, S, O_2.)"

C13. L65: integrated? over the entire ocean? Please clarify.

A: Agreed, this was unclear to the reader. Integrated referred to TOU+$O_2^{dis,bio}$, not any regional integration. We changed from 'integrated ocean' to 'marine' (L77).

C14. L85: "We implemented": Does that mean the authors added the functionality (for simulating preformed tracers) to the model? Or simply that they used an existing functionality in the model? Please clarify.

A: [L 103ff in revised docx.] Yes, this functionality was actually added ('implemented') by the first author to the UVic 2.9 code, following similar suggestions used in other models (Ito, Bernardello reference). Only some of the respective tracers (e.g. TOU in Koeve et al., 2020; DICremin, DICpre in Koeve et al., 2024) have been used in our earlier work. Given that the reviewer refers to a paragraph point here, with details given below, we think that our terminology is correct.

C15. L85–90: The presentation of preformed tracers can be simplified. For example, what about:

The preformed tracers DIC^{pre}, O_2^{pre}, and PO_4^{pre} represent the DIC, O_2 , and PO_4 that would be transported and mixed conservatively from the surface into the interior without any interior sources or sinks.

A: Changed accordingly. (L 105ff of revised ms)

C16. L90: A common name for these "remineralized" tracers is "regenerated". I understand this might not be the preferred term for the authors, but it is widely used in the literature when decomposing tracers into preformed and regenerated components.

A: In the course of writing the manuscript we have had ample discussions with respect to what terminology to use to refer to the accumulated products of organic matter degradation. We agree using one terminology or another can be confusing. There is a diversity of terminology used in the literature (DIC^{reg}, DIC^{soft}, DIC^{remin}) with diverse meaning, notably by the same authors changing over time. Our thinking/line of arguments was the following:

Why we do not use DIC^{reg}: E.g. Ito et al. 2004 use DIC^{reg} for carbon from degradation of organic matter (in line with the reviewers suggestion), however in a later publication Ito et al., 2015, https://doi.org/10.1002/2015GL064320, include carbon from $CaCO_3$ dissolution into the term DIC^{reg} as well. Also e.g. Bernardello et al., 2014 have used the term DIC^{remin} for the sum of DIC from organic matter degradation AND $CaCO_3$ dissolution. Because of the ambiguity of inclusion of $CaCO_3$ dissolution versus not, we find for the purpose of our study C_{soft} to be more precise.

How we use DIC^{remin}: We further note that we use the term remineralized in our study to denote specific tracers related to organic matter degradation AND which are set to zero in the surface ocean (i.e. our tracers DIC^{remin}, PO_4^{remin}). This tracer does not include the eventual resubduction of degradation products and thus we argue is incomplete in describing interior ocean enhancement of carbon due to degradation products stemming from organic matter. We hence stick to the term "remineralized" for our specific tracers (DIC^{remin}, PO_4^{remin}).

Why we use C^{soft}: It is the term which we think is used most widely and specifically to describe carbon from organic matter degradation (Eggleston & Galbraith, 2018, https://doi.org/10.5194/bg-15-3761-2018; Wilson et al., 2022; Kwon et al., 2011, https://doi.org/10.1029/2011GB004059; Bernardello et al., 2014; Kwon et al., 2009, http://www.nature.com/doifinder/10.1038/ngeo612; Sarmiento and Gruber, 2006, Ocean biogeochemical dynamics). We hence reserve the term C^{soft} in our study to describe generically carbon from the decomposition of organic matter, irrespective whether it was formed after or before the last contact with the surface.

While writing the manuscript we found it necessary to have such a generic term, which is distinct from terminology we use to describe specific approaches, like our tracer DIC^{remin} or the AOU-approximation to quantify this component.

C17. L108: This is incorrect :
Within the interior ocean DIC^{sat} is a passive tracer, i.e. has no sources or sinks. Passive tracers are tracers that don't participate in the dynamics, as opposed to active tracers, like temperature and salinity, which affect density. I think the authors mean "conservative" here.

A: Changed accordingly (Line 126 of revised ms)

C18. L153: As for L108, I think the authors mean "conservative" here .
That is, computing O_2^{sat} can give a smaller value compared to O_2^{sat} from an explicit O_2^{sat} tracer which is computed at the surface given surface temperature and salinity and mixed passively in the interior ocean.

I think the key word that is missing here is "non-linearity": O_2^{sat} is a nonlinear function of T and S, so that even if T and S were transported conservatively, O_2^{sat} itself may not be conservative. This is a common issue with "derived" tracers, such as potential temperature, which is not exactly the same as conservative temperature (e.g., Jackett et al., 2006).

A: We changed as follows
"That is, computing O_2^{sat} **from mixed temperatures and salinities** can give a smaller value compared to O_2^{sat} from an explicit O_2^{sat} tracer which is computed at the surface given surface temperature and salinity and mixed **conservatively** in the interior ocean. This effect, **which is associated with the non-linear relationship between T, S and O_2^{sat},** however, is, globally, much smaller compared to the disequilibria evaluated in this study (Fig. S9) and ignored henceforth."
(L 238ff of revised ms).

C19. L169–178: This paragraph is confusing to me. It is about how difficult it is to compute $DIC^{dis,bio}$ compared to $O_2^{dis,bio}$.

A: We admit that we kept this paragraph initially (too) short and used partly unclear terminology, see details below

a) The explanation of the difference is confusing. Is it the reason for the difficulty the size of the atmospheric and ocean reservoirs? Or is it the fact that DIC affects atmospheric CO2 and the climate? Or both? This is unclear in the current phrasing. Please clarify.

A: Concerning (a), it is both. In 259ff of the Methods section of the revised ms we now clarify:

"Attributing DIC^{dis} to physical versus biological effects is not as straightforward as for O_2, as there is no meaningful definition of an abiotic DIC tracer in an Earth system model **experiment** where atmospheric CO_2 is affected by ocean biology. For O_2 the atmospheric reservoir is large **(about 100 times the current marine O_2 reservoir; Duursma et al., 1994)** and changes in marine oxygen can be neglected when computing the atmospheric oxygen boundary condition of the ocean. This is very different for carbon, **where the ocean holds about 60 times as much carbon as the atmosphere, changes in ocean carbon are known to affect atmospheric carbon (e.g. Maier-Reimer et al., 1996) and thus** climate **(which is not the case for oxygen)**."

b) What is different between the "normal" model and the "AllPumps" model? Aren't all the marine carbon pumps turned on in the default setup? Is that an "additional" experiment? Please clarify.

Concerning (b), we clarify that our "normal"/default model configuration is the AllPumps model, with all marine carbon pumps turned on in this configuration. We now avoid the unclear term 'default setup' where possible, clarify where we use the allPumps and where the noBioPumps configuration. The term 'default model' configuration is now only used to distinguish against the sensitivity experiments with modified parameter sets further down in the ms. (In L335 ff of the revised ms, we write).

"In the following we use the term 'default model' when using our default biogeochemical and physical model parameter set (which is based on former model tuning to observations; Keller et al., 2012) in the allPumps configuration, opposed to sensitivity experiments described in the next paragraph."

c) In the "NObioPumps" model, the atmospheric CO_2 is still allowed to evolve but the total (ocean + atmosphere) carbon is conserved, presumably at the preindustrial level. But then that means that the surface DIC and atmospheric CO_2 are higher, so the climate is warmer, and so the ocean is also warmer, and the circulation is different, and so on. Please clarify exactly how the steady-state "NObioPumps" model looks like. In particular its climate and its ocean circulation. This seems important to interpret the results.

Concerning (c): To clarify: In noBioPumps atmospheric CO_2 responds to changes of the ocean carbon inventory, conserving the total (ocean+atm) CO_2 inventory at the preindustrial level. To clarify how we handle the experiments with anthropogenic CO_2 emissions and climate change to avoid additional effects from climate change during model spinup and transient we have added the following text (Line 308ff).

" Both model **configurations** are run into steady state, conserving the atmosphere+ocean total CO_2 inventory (mass conserving approach; **see also Tjiputra et al., 2025**) **and sharing the same radiative forcing of allPumps**."

d) The authors state (here and later in the results) that “there is no meaningful definition of an abiotic DIC tracer”. What “meaningful” means here is unclear and appears to be in direct contradiction with what the authors actually do, which I consider to be meaningful. So I disagree with this statement, even if it is open to interpretation. I would suggest avoiding general statements like this one and simply explaining the factual reasons behind the authors’ methods. I don’t understand why the “NObioPumps” model’s DIC is not a meaningful definition of an abiotic DIC tracer, since it is exactly that: a model configuration where the biological pumps are turned off, so that the ocean carbon cycle is purely abiotic. Please clarify.

Concerning (d) we originally wrote “there is no meaningful definition of an abiotic DIC tracer in an Earth system model where atmospheric CO_2 is affected by ocean biology “. As we know from earlier studies (e.g. Maier-Reimer et al., 1996, *Clim. Dyn.*, *12*, 711-721), an Earth system model with an ocean without biology has a considerably higher atmospheric pCO_2 compared with the contemporary atmosphere. This evolving atmospheric pCO_2 is not known in a model with biology and hence cannot be used as a boundary condition for an ocean abiotic tracer, DIC^{abiot}. Technically, one could add a second tracer of CO_2 in the atmosphere (a tracer which interacts with the ocean abiotic tracer, DIC^{abiot}, but that does not affect climate). We instead decided for the alternative (scientifically equivalent) option of using an additional model setup in which biology does not affect the carbon cycle, simply since it comes with a number of technical advantages. The major one is that we can carry out different experiments with different configurations of the noBioPump model with respect to the pCO_2 boundary and its impact on climate. In the present study we choose the nioBioPumps configuration in which atmospheric pCO_2 is not fixed but can evolve (keeping the ocean+atmosphere carbon inventory constant at preindustrial levels during the spinup), but we keep the climate consistent with the allPumps model, that is apply the same radiative forcing as in the allPumps model (see c). To clarify our reasoning, we amended the sentence by the word ‘experiment’ and avoid the ‘term meaningful’ and now write (L 259f):

“there is no straightforward definition of an abiotic DIC tracer in an Earth system model **experiment** where atmospheric CO_2 is affected by ocean biology” (L 214)
and
“This indirect approach is needed since there is no **straightforward** definition of an abiotic DIC tracer …“ (L 598)

C20. L195: A 17-character-long superscript is, to say the least, a little unwieldy. Surely it’s possible to come up with better notation here. I also must admit I dislike the COU* and BGC* notation. I am actually having a hard time tracking these convoluted model configurations. I also don’t see the point of the “COU” (and “AllPumps”) notations at all, since these are the default configuration. A table/matrix or a diagram showing what is turned on/off in each model configuration would be very helpful

A: It is not all the same as we try to clarify in the following. AllPumps and noBioPumps are different model configurations needed in the carbon section to compute by difference

$DIC^{dis,bio}$. We clarify now in the methods section that AllPumps is the same model configuration used in the oxygen section (L 265ff):

"Computation of the biotic component of DIC^{dis}, $DIC^{dis,bio}$, is thus based on **dedicated** experiments carried out with two **configurations** of our model, one configuration with all marine carbon pumps turned on (allPumps**, which is identical to the model configuration used in the oxygen section**) and one in which the impact of biological carbon pumps on alkalinity and DIC is disabled (noBioPumps). "

Both model configurations are first run into their own steady state and thereafter forced with our idealized climate change scenario (see L 270-307).

The terminology COU* and BGC* relates to different configurations of the transient simulations (hence COU* is not the same as allPumps, but a specific experiment carried out with the configuration allPumps AND with the configuration noBioPumps, and BGC* is another set of experiments carried out with both configurations.) (L320 ff)

This transient experiment is performed with the allPumps and the noBioPumps model **configurations** in two setups. In the **first** setup (COU*) the model is fully coupled, i.e. CO_2-emissions cause global warming and climate and circulation change. In the second setup (BGC*) the climate is kept constant at the preindustrial state.

We use the term COU further for consistency with terminology used in the C4MIP "feedback community" since the C4MIP protocol provides very helpful approaches to distinguish different processes in transient model experiments.

We further clarify (L337ff):
"In the following we use the term 'default model' when using the our default biogeochemical and physical model parameter set (which is based on former model tuning to observations; Keller et al., 2012) in the allPumps configuration, opposed to sensitivity experiments described in the next paragraph. "

We agree that the abbreviation introduced in Equ. 10 (former Equ. 9) is quite long. We suggest to use instead $\Delta DIC^{dis,bio,climate}$, i.e.

" $\Delta DIC^{dis,bio,climate} = DIC^{dis,bio,COU*} - DIC^{dis,bio,BGC*}$ (10) "

Finally, we note that we will provide a clarifying table to characterize the different configurations and experiments.

C21. L222–224: Sentence is hard to parse and confusing. Why mention cooling in this context? Shouldn't we assume this means this is a steady state, such that there is no cooling? Please clarify and simplify.

A: We are sorry for the confusion. We have clarified and simplified the phrasing:

"(L392) Globally, most of this difference, about 80%, is explained by upwelling of undersaturated waters that **are low in O_2 due to** organic matter degradation in the interior ocean (Fig. 1a). This 'biological' contribution **is quantified by** $O_2^{dis,bio}$ (Fig. 1a, Fig. S2**; Eq. 4**). … (L 397) **The**

remaining 20% of the globally integrated O_2^{dis} is explained by the thermal effect of solubility increase due to cooling during the process of dense water formation and subduction (Fig. S2). "

We hope this clarifies the meaning of cooling in the context of a steady state situation.

C22. L229: "estimate of estimates of" → "estimates of"
A: done

C23. L239: Why is "biological" in quotes?
A: quotes deleted

C24. L239–242: O2 dis,bio and co were already defined in the methods section, so just use them.
E.g., replace

"We refer to this 'biological' contribution to the total O disequilibrium as Odis,bio (...)"

with

"This 'biological' contribution is quantified by Odis,bio (...)"

A: Simplified, the sentence reads now (L394):
"This biological contribution is quantified by $O_2^{dis,bio}$ (Fig. 1a, I; Equ. 4). "

C25. L241: Why is abiotic in quotes?
A: This part of the sentence was deleted, as suggested in C24.

C26. L244: If the authors agree to avoid the term "O2 debt", they could simplify

"where waters with a large O2 debt from organic matter degradation return to the surface"

to something like

"where waters biologically stripped from their original O2 return to the surface"

A: adopted (L 397 of revised ms)

C27. L245–147: This is an incorrect (or at least ambiguous) definition for TOU:
"TOU is an appropriate measure of the accumulated O2 utilization of interior ocean waters since last contact with the atmosphere"

No, TOU is the accumulated O2 utilisation of interior ocean waters since last contact **with the surface layer** (rather than with the atmosphere). That surface layer is exactly the volume in the model where the Opre tracer is reset to total O (and TOU reset to zero).

A: changed accordingly (L402)

C28. L245–248: This is also incorrect :

“The fact that AOU is larger than TOU (Fig. 1a, c, e) in magnitude indicates that (...) TOU is (...) in fact not a good measure of the total accumulated O2 debt associated with the degradation of organic matter in the interior ocean”

The point of TOU is precisely to account for the **true** oxygen utilisation in the ocean interior. The fact that AOU is not equal to TOU is by no means an indication that TOU underestimates or overestimates oxygen utilisation; it only indicates that AOU is an inexact approximation of TOU.

A: We realized that we phrased this not clearly. Our main point is not that one measure is a better estimate than the other for the same quantity, but that *TOU and AOU provide different sorts of information*. To clarify this, following also reviewer 2, we emphasize this now early on in the manuscript (Abstract, Methods). Since TOU (the so called ‘true oxygen utilisation’, Ito et al. 2004) is set to zero when a water mass had last contact with the surface (C27), it accounts solely for biologically stripped oxygen of a water parcel since it was subducted from the ocean surface. AOU in addition includes any biologically stripped oxygen prior to that, due to incomplete equilibration of upwelling oxygen deficit (the unequilibrated part is dubbed $O_2^{dis,bio}$ here). Our interest is in the total interior ocean O_2 depletion and DIC enhancement due to biological degradation processes, hence combination of the quantities/measures, $TOU+O_2^{dis,bio}$. To try to make this notion explicit we refer to now as O_2^{util}.

To improve the logic of the paragraph the reviewer is referring to we have rephrased to: [L 401ff]

“**W**hile TOU is an appropriate measure of the accumulated O_2 utilization of interior ocean waters *since last contact with the* ***surface***, it is in fact not a good measure of the total accumulated **biologically-stripped** O_2 in the interior ocean.”

C29. L248–249: This is incorrect :

“AOU, in turn, adds the information of the effect of biologically induced undersaturation of waters at the beginning of their next journey through the interior ocean. AOU equals TOU+O2dis,bio (...). It hence provides a very reliable global state 2 estimate of the total biological effect on O2, and we will discuss in the following to which extent AOU agrees with TOU+Odis,bio in a changing climate and for other variants of our model.”

AOU does not “add” any information. I understand the authors mean that AOU is affected by the state of oxygen saturation when waters leave the surface layer, which would be correct, but that is not what the sentence says.

This goes back to my general comment **GC2**: Without a clear definition of what the “total effect on O2” means, the statement is just an empty assertion on undefined terms. I think using my recommended definition for O2bio would make this much clearer and more precise.

A: As stated above (our response to GC2), we fear that defining TOU+$O_2^{dis,bio}$ as O_2^{bio} comes at the risk that readers may confuse it with the oxygen produced during photosynthesis. However, we have rephrased here and make use of the term O_2^{util} introduced now in the Methods section.

We have rephrased to (L 403):

“**This is so, since TOU misses** the information of the effect of biologically induced undersaturation of waters at the beginning of their next journey through the interior ocean. AOU equals **O_2^{util} (i.e. the sum of** TOU and +$O_2^{dis,bio}$, Eq. 5; Fig. 1c, g) globally with only 2% uncertainty (Fig. 1a) in our default model setup ….”

C30. L259: What about “of the former difference” → “of ΔAOU” for clarity?

A: we rewrote to (L 414):
“of the difference **between *Δ*AOU and *Δ*TOU** “

C31. L268: “from” → “with”

A: done

C32. L314–317: This is convoluted and **very** unclear:

“We refer to the fraction in DIC which can be attributed to the soft tissue pump as ‘soft tissue pump carbon’, Csoft. In analogy to our findings above, we consider Csoft to be a composite of (a) Csoft accumulated since last contact with the atmosphere and (b) a recirculated biotic component of the carbon disequilibrium.”

What is Csoft? It is not really defined here. Saying it is “a composite of (a) and (b)” is open to interpretation: What is a “composite”? Worse, (a) includes Csoft itself. Thus even if “composite” was clearly defined, this would still be a circular definition. Furthermore, “a recirculated biotic component of the carbon disequilibrium” is quite convoluted, for no reason.

A: We clarify and rewrite to (L 529)

“In analogy to our findings above, we consider C^{soft} to be t**he sum** of (a) **remineralized carbon** accumulated since last contact with the **surface** and (b) a recirculated biotic component of the carbon disequilibrium (Equ. 11). “

Much clearer would be **an equation**! I assume that it is

Csoft = DICremin + $DIC^{dis,bio,COU*-BGC*}$,

given the parenthetical in the abstract. This equation (or the correct equation if mine is incorrect) should be in the main text. And DICdis,bio,COU*-BGC* should be removed from the abstract because it’s too obscure without a proper definition, which the abstract is not for. I also reiterate the comment I made in **C19.d**.

A: Introducing this equation at this point appears premature in our view. It needs the following 1.5 pages of manuscript to arrive at this point (for transient climates. We add the following text and equation at the end of the first paragraph of section 3.2 (L 595):

“For steady state situation with quasi-constant atmospheric pCO_2 boundary condition, we propose to quantify C^{soft} as:

$$C^{soft,\ steady\ state} = DIC^{remin} + DIC^{dis,bio} \quad (11)“$$

and use C^{soft} instead of DIC^{remin} + $DIC^{dis,bio}$ in the paragraphs to follow. Close to the end of section 3.2 we add the following (L 630 ff):

“Returning to our earlier argument that we consider C^{soft} to be the sum of (a) remineralized carbon accumulated since last contact with the atmosphere and (b) a recirculated biotic component of the carbon disequilibrium, we propose that the change of C^{soft} is best estimated by the sum of ΔDIC^{remin} and $\Delta DIC^{dis,bio,climate}$, i.e.

$$\Delta C^{soft} = \Delta DIC^{remin} \text{ and } \Delta DIC^{dis,bio,climate} \quad (12) “$$

Figure comments

A: **We agree that the figures benefit from improvements and will do so accordingly for the revised version of the manuscript.**

C33. Remove author signature overlaid on the side of almost every panel in every figure (main and supplement).
A: See general statements concerning figure improvements above.

C34. Colormap choices: Use diverging colormaps for diverging data (disequilibriums) and sequential colormaps for sequential data (AOU, TOU, etc.) Applies to
- Figure 1c–j.
- Figure S1a (and possibly c and d but cannot tell from hidden titles/units).

• Figure S2a–c (all panels).
A: See general statements concerning figure improvements above.

C35. Figure 1:

a) Remove nonsensical x-axis ticks in panels a and b.
b) Remove author signature overlaid on the side of every colorbar
c) panel j title is cropped
d) The “transect” is a little confusing: Atlantic goes from 70°N to 80°S, but then there is a 60°S indicated over the Antarctic, and then back to 80°S for the Pacific. Maybe a map can be added in the supplement to clarify the “transect” line?

A: See general statements concerning figure improvements above.

C36. Figure 2:

a. panel a:

i. “ΔO_2^{util}” in the y-axis label has not been defined. Just use “ΔO_2 ” since it’s an axis for changes in oxygen, and not all of them are about utilisation anyway
ii. Title is also incorrect
iii. “Temporal development” → “Time series”
iv. compliment: labels next to the data is great!

A: See general statements concerning figure improvements above.

b) panels b and c: Place labels next to the data like in panel a.

A: See general statements concerning figure improvements above.

c) all panels: second y-axlis ticks can be removed.

A: See general statements concerning figure improvements above.

C37. Figure 3:

a) Add some color and a legend or some text labels to help the reader.
b) Format tick labels the same (currently a “.0” appears only on the y-axis)
c) Use better titles: they are all technically “state estimates”, so panel (a) title is unhelpful, and the last 2 are identical, which is also unhelpful.

A: See general statements concerning figure improvements above.

C38. Supplementary figures: It would be convenient and helpful to place the captions with the figures instead of on separate pages.

A: See general statements concerning figure improvements above.

C39. Figure S1:

a) White band covering panel c) title means we don’t know what is plotted.
b) White band covering panel d title means we are unsure if it is just Opre that is plotted.
c) Remove author signature overlaid on the side of panel c
d) Remove author signature overlaid on the side of panel d

A: See general statements concerning figure improvements above.

C40. Figure S3: x-axis ticks labels are too small and formatted with unnecessary leading zeros.

A: See general statements concerning figure improvements above.

C41. Figure S6: Add a legend or some text labels to interpret the different time series at a glance.

A: See general statements concerning figure improvements above.

C42. Figure S7: place labels next to data.

A: See general statements concerning figure improvements above.

C43. Figure S8: x-axis ticks labels are too small and formatted with unnecessary leading zeros.
A: See general statements concerning figure improvements above.

Final Remarks

I think this is a very nice and interesting study that will benefit a wide audience once it has been polished and published in Biogeosciences. Congratulations to the authors for thinking about AOU and TOU in this novel way.

Note: The reviewer provided also a checklist table (see: https://egusphere.copernicus.org/index.php?_mdl=msover_md&_jrl=778&_lcm=oc108lcm109w&_acm=get_comm_sup_file&_ms=137710&c=310152&salt=17851478951304994034), which I do not copy here.

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A: Again, we like to thank Benoit Pasquier for his time and effort and this very constructive and helpful review.
