

Below we have pasted the Editor's and Reviewers' comments in their entirety interspersed with our responses in blue font.

Editor Comments:

Dear Authors,

We have now received both second round of comments from the reviewers. One of the referee was more critical in his first round of comments. You can now see that he would accept the paper with "minor revision", basically stating more clearly what is your position on the importance of subduction in the mixed layer (and below) at the time scale you are working, and why you think this is a reasonable assumption (the Rysgaard vs. Moreau/Grimm conundrum).

Could you please amend your manuscript along those lines, and send me a revised version, both "unaltered" and with "annotated" changes, I will then make a final decision,

I think we are getting there, apologies again for the delays...

Best regards,

Jean-Louis Tison

Dear Dr. Tison,

Thank you for giving us the opportunity to clarify our stance on subduction. We recognize that we were lacking a clear description of how we deal with it and are hopeful that this modified version fills the gap.

Best,
Benjamin Richaud on behalf of all authors

Comments from Reviewer 1:

Im happy with the reviewers comments, changes and answer to my initial review.

Response: Thank you!

Comments from Reviewer 2:

Dear authors,

Thanks for reading my comments, and sorry I took so long to come back (all my fault, but too much work over the last two months).

Response: No worries, the last few months were busy on our side as well, so we perfectly understand!

I think a very short amount of extra-work related to subduction.

To rebut my argument on subduction, you say you are focussed on air-sea fluxes at seasonal to inter-annual time scales, whereas some of your plots cover 150 years, and your conclusions on carbon uptake (in the abstract) probably regard integral processes that involve the full carbon budget in the mixed layer... I did not find that very convincing, but I think that was not really addressing what I had in mind (and possibly did not express very clearly).

Response: We agree with the reviewer that there is a limitation to our study with regard to the consideration of subduction. As previously stated, on the seasonal time scale, we believe our parametrization is justified. However, when making inferences over the 150 year period, our estimate should be considered an upper bound and we have tried to clearly state this in the discussion as follows (l. 398; added text in bold font):

“The output from the ACCESS-ESM1.5 model accounts for subduction, but the fate of supplementary carbon estimated here cannot be determined without a proper coupling of a sea ice biogeochemical component. It is therefore unknown whether, **at the decadal time scales considered for that model**, carbon flux driven by advection and mixing would proportionally increase and export the supplementary carbon or whether the latter would saturate the surface mixed layer, leading seawater $p\text{CO}_2$ to catch-up with atmospheric values faster than without accounting for the sea ice carbon pump. **Thus, our estimate should be considered an upper bound of the impact of the sea ice carbon pump.**”

I think what Moreau & Grimm show is that subduction is very small contributor at short time scales, whereas it is assumed 100% efficient in Rysgaard model. Therefore, what I missed in your model description, is a clear statement on what you do on this assumption, hoping that you are close to neglecting this (which would be fine even on decadal time scales)... Are you on Rysgaard's line (assuming full subduction), or on Moreau/Grimm's line (weak net subduction) ?

Response: We do not follow Rysgaard's assumption of full subduction. We believe this is now clearly stated in both the methods where we added the following sentence (l. 223):

“As a consequence, subduction processes are mostly but not entirely neglected in this 1D model.”

and in the discussion with the following edits (l. 379, new text in bold):

“Our estimated supplementary carbon flux is consistent with numbers given by Rysgaard et al. (2011) who suggested that the sea-ice carbon pump could represent 20 % of the air-sea CO_2 flux in open Arctic waters at high latitudes. **Rysgaard et al. (2011) assumed complete subduction of the brine, while we did not.** Our estimates are higher than those from two other modelling studies. Grimm et al. (2016) reported that 7 % of simulated net polar oceanic CO_2 uptake is due to the sea ice carbon pump. Moreau et al. (2016) found a weakened Arctic carbon sink when including the sea-ice effect. **Neither of these two studies assumed complete subduction and rather diagnosed it from their model, finding it to be relatively small. It has been previously suggested that the differences between the estimates of Rysgaard et al. (2011) and Moreau et al. (2016) are due to the different assumption about subduction. This study does not support that interpretation.** While a direct comparison **between all** with those studies is difficult, we suggest that the vertical resolution is crucial for properly resolving the mechanisms at play.”

With a proper statement on that matters I think your paper could go.

All the best,

Martin Vancoppenolle

Thank you, we are grateful for the comments and believe the manuscript is improved thanks to those.