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Interactive comment on "Remineralization rate of terrestrial DOC as inferred from CO_2 supersaturated coastal waters" *by* Filippa Fransner et al.

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We want to thank referee 2 for his/her work and the helpful comments that will improve our manuscript. Below you find a response to each comment. The referee's comments are marked in bold and our responses are to be found just below.

1. I think it would be helpful to have a map of surface salinity, either seasonally resolved or as a monthly climatology. This would help the reader to link the maps and the scatter plots of pCO2 against salinity. This could even be in the supplementary material.

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This is a good idea, we will add it in the supplementary material.

2. What is the data source for the riverine carbon and other chemical fluxes? I presume that the river runoff from EHYPE refers to the freshwater flux rather than the chemical fluxes, or did I misunderstand that?

Indeed, the river runoff from EHYPE only contains freshwater fluxes. The chemical fluxes have been calculated from measurements of river concentrations together with the freshwater fluxes from EHYPE as in Fransner et al 2018. We will clarify this in the manuscript.

3. I agree that Figs 4 and 5 show that the 1Y model comes closest to reproducing the pCO2 observations. However, it seems to me that there are quite a lot of very high pCO2 data that are not predicted by any of the models (esp. in Mar, Apr, and May). Could you maybe add some discussion, even if speculative, about what might be causing even higher pCO2 than in the model?

We agree that this should be discussed in deeper detail and will therefore add it in the manuscript. One explanation can be that we have a relatively simple degradation model assuming that the tDOC only consists of two pools of different lability. In reality there could for example be one additional pool that is degraded with a faster rate than we use and that is consequently quickly removed in the low salinity region, with a larger impact on the pCO2.

4. In Section 3.3, I see what you mean by the 10Y remineralisation rate in Fig 6 showing a more spread-out pattern than 1Y. However, in Fig 5, the lines of 10Y and 1Y are almost identical, except below salinity 3 in Jan–May. Why is there no clearer impact on the pCO2?

This is a very good remark! It is true that the pattern of the remineralization rate and the pCO2 difference (related to the 10Y experiment) in Figure 6 is not very identical. If you compare Figure 6b to the bathymetry of the domain (Figure 1b) you will see that that

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the column-integrated remineralization rate is higher in the deeper parts of the domain. This means that the remineralization of the terrestrial DOC is more spread out over the whole water column, and that there is also remineralization taking place below the thermocline/halocline, which does not directly impact the surface water pCO2. We will add a description of this in the manuscript. We also see that we haven't explained that the difference in the pCO2 only refers to the surface water pCO2, and we will correct this.

5. I'm less convinced of the estimates of remineralisation time-scales that the authors calculate on page 8. They are using a simple exponential decay model that assumes that the entire tDOC pool is potentially labile, and then take the concentration reported for the final time-point in each incubation to calculate the time-scale. I've not had time to look through the references myself, but degradation experiments like these typically take measurements at multiple time-points. I think the authors should really confirm by checking the cited papers again that a single decay model without an asymptote really is justified for each case, as opposed to a more complicated exponential decay model in which one fraction is labile and one fraction is refractory. Maybe the original data from these incubations could even be re-plotted as a supplementary figure with the present authors' decay model superimposed. If the original data do not agree well with the exponential model proposed here, then the authors should discuss possible reasons why microbial remineralisation might be more active in the environment than seen in incubations (maybe priming? Differences in microbial community?).

We agree that our model of remineralization is very simple (which could partly be an explanation to your question number 3) and that there are more sophisticated models that can resolve different pools of DOC with different lability. Indeed, some of the references that we show in the table show a time series of a relative change or actual concentrations (Herlemann et al. 2014, Asmala et al 2014, Hulatt et al. 2014).

It would be interesting to plot everything in one figure, but we believe that extracting the concentrations from the figures with several sampling points (in time) would be too difficult (the DOC concentration-axis is not highly resolved enough to do it by eye). However, we agree that this deserves a discussion, which we didn't have in the first round of our manuscript. We will therefore add a discussion on different remineralization models and the uncertainties associated with our model, in comparison to the kisted references.

6. Page 8 bottom line: the units are incomplete for the CO2 uptake rate, and in both cases it should read "m-2" instead of "m2".

Thanks, we will fix this.

7. Page 9 line 10: I got confused here when the authors refer to "CO2 uptake" in the Bothnian Bay, since they say before that the entire Bothnian Bay is a CO2 source to the atmosphere. This needs either correction or better explanation.

We will change this to air-sea CO2 exchange, where negative values indicate an outgassing, and positive values indicate an uptake.

References:

Fransner, F., Gustafsson, E., Tedesco, L., Vichi,M., Hordoir, R., Roquet, F., Spilling, K., Kuznetsov, I., Eilola, K.,Mörth, C.-M., Humborg, C., and Nycander, J.: Non-Redfieldian Dynamics Explain Seasonal pCO2 Drawdown in the Gulf of Bothnia, Journal of Geophysical Research: Oceans, 123, 166–188, https://doi.org/10.1002/2017JC013019, https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1002/2017JC013019, 2018.

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