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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.

**Anonymous Referee #2**

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The manuscript by Fransner et al. presents an analysis of pCO2 data from the Gulf of Bothnia together with a biogeochemical model of the basin. Several model scenarios are presented that make different assumptions about remineralisation of tDOC. The authors thereby show that the high pCO2 values in the observational data are only consistent with a model in which tDOC is remineralised rapidly by microbial processes, and in which tDOC also increases light attenuation and thereby reduces primary production close to the coast.

In my opinion, this manuscript presents an insightful analysis that helps us understand the important question of the fate of tDOC in the sea. The manuscript is well written,



clearly structured, and the data are presented clearly. While I am no expert in biogeochemical modelling, their model scenarios seem to me to be appropriate for testing their hypotheses, and I believe that their conclusions are justified by the results.

I therefore only have minor questions and comments for the authors to consider. These are as follows:

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.

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?

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?

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?

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?).

6. Page 8 bottom line: the units are incomplete for the $CO_2$ uptake rate, and in both cases it should read "m-2" instead of "m2".

7. Page 9 line 10: I got confused here when the authors refer to "$CO_2$ uptake" in the Bothnian Bay, since they say before that the entire Bothnian Bay is a $CO_2$ source to the atmosphere. This needs either correction or better explanation.

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