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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First of all we want to thank referee 1 for his/her comments that are very constructive and will help improving the manuscript! Below you will find a response to each comment. The referee's comments are marked in bold and our responses are to be found just below.

1) The authors use a quite complex biogeochemical model (BFM) which describes the planktonic ecosystem through a numbers of different plankton functional types. The latter include explicit bacteria and two species of DOM (labile and semi-labile). However, when considering tDOC, the authors use a simplistic

decay function assuming that tDOC is all consumed in 1 or 10 years. Why tDOC was not assumed to be cycled by bacteria which are already modeled within the BFM? By using a fixed decay constant, remineralised tDOC goes directly into the DIC pool which is a simplification. Indeed the bacterial growth efficiency in estuaries and coastal zone is relatively high (del Giorgio and Cole 1998) implying that a substantial fraction of DOC assimilated by bacteria is incorporated into bacterial biomass. This might affect the ecosystem in various ways e.g. by affecting grazing (HNAN), the competition between bacteria and phytoplankton for nutrients and the production of recalcitrant DOC. It is also strange that the authors seem to use a different approach for the riverine POC which is assumed to be used by bacteria in the same way as marine POC. I think that the different approach-i.e. the lack of explicit bacterial utilization- used for the experiment with tDOC (the one leading to the main result of the paper) needs to be discussed and justified.

The referee is right that we haven't explained clear enough the reason behind our approach, and we will make this clearer in the manuscript as described below: The reason to why we have chosen to use a simplistic decay function for tDOC was firstly to be consistent with the Fransner et al. 2016 paper, on which this study is based. In this paper the decay rate of tDOC is investigated and compared with actual estimates of tDOC concentrations in the estuary, and it is found that the decay can be modeled by either using a rate of 1 year on 80% of the tDOC, or 10 years on 100% of the tDOC. The idea of this study is primarily to investigate which of these rates (if any) that are more realistic when comparing modeled pCO2 to the observed one. In other words the main aim of this paper is to answer the question "Is remineralization (by bacteria and/or sunlight) an important removal pathway of tDOC in the Gulf of Bothnia, and in that case, on what time scale does it occur". The discussion that follows about the underlying process (bacterial/photo-remineralization) is more of a secondary result. We will clarify this in the manuscript.

We agree that it would be interesting to actually let the bacteria degrade the tDOC and to investigate how this affects the competition with phytoplankton. However, we think that when doing so the model output should also be compared to bacterial biomass and growth rates, and that would be enough material for a paper on its own. We therefore think that this is out of the scope of this paper and that it would be an interesting follow up paper. We will write a paragraph on "future studies" where we discuss this in our manuscript.

The reason to why the POC is utilized by the bacteria is that it is a built-in feature in the BFM model (the terrestrial DOC was added by ourselves). As the tPOC concentrations are very low compared to the tDOC concentrations, it doesn't have significant impact on our results. This will be clarified in the manuscript.

2) Equation 1(1). This equation is not very clear to me: If Kdtdoc represents the contribution of tDOC to the total light extinction, it should have the same units as the total Kd (i.e. m-1, as presented in Fig 3). The units reported at line 30 of page 4 seem to refer to the specific adsorption coefficient (see equation 9 in Vichi et al 2007) which (I guess) is represented by the parameter '1.0' multiplied by tDOC in eq 1.

The referee is correct, here we have made a mistake. Kdtdoc represents the contribution of tDOC to the total light extinction and should have the units (m-1). We will correct this in the manuscript.

3) tDOC is given in ug m-3 which is quite unusual for marine DOC (usually given in mmol m-3) this of course is not a big problem but from eq 1, tDOC concentrations seem to be very low (assuming a max value of Kdtdoc of 7.5). What is the concentration of tDOC given as input to the model? And what is the concentration of the simulated total DOC?

Also here we have made a mistake; the labile tDOC concentrations amounts to 7500 mg m-3 in the model. So equation number 1 should be written:

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$$k_{d_{tDOC}} = 0.15 + 10^{-3} tDOC$$

This will be corrected.

4) Why Kdtdoc is not equal to 0 when tDOC is zero?

This is to take into account the contribution of the refractory fraction of tDOC to the light extinction coefficient, which is not modeled explicitly in the 1Y and the 1YS experiments (see also your comment number 6). We will make this clearer in the manuscript.

5) The authors cited different papers reporting different light extinction coefficients (differing by more than one order of magnitude). This suggests that the parameters used to simulate kd are very uncertain. I think that a sensitivity analyses would be useful to understand how the presented results (relative to the exp. 1YS) are affected by the choice of the specific light absorption parameters.

When investigating this we also did simulations where the light extinction coefficient didn't reach as high as 7.5 (and reached values about one order of magnitude smaller, as in the cited papers). With this the modeled pCO2 drawdown was still too high compared to observations in the low salinity-region. The aim of this experiment was only to provide a possible explanation to the high pCO2 values also during the productive season. More research (and simultaneous measurements of DOC and PAR) are needed to get a deeper insight on the effects of tDOC on the light extinction and primary production. We will add some text discussing this in deeper detail.

6) Only the labile fraction of tDOC is assumed to contribute to light extinction. However the biologically refractory fraction of tDOC can be composed by aromatic compounds which strongly interact with light (e.g Stubbins et al. 2010, LO)

See answer to your comment number 4.

7) No mention of the model skills in reproducing broad ecosystem variables (apart from DIN, DIP and pCO2) and fluxes. For example, is the primary produc-

tion simulated in the various experiments realistic? Are there data available for comparison? If not, simulated values of Chl and primary production could be at least discussed in the context of what is observed in similar areas/ecosystems. I appreciate that the authors refer the reader to a previous paper for a complete validation of the model, however, it would be nice to see a summary of that validation in this manuscript. Additionally it would be very useful to see how the model performance varies in the different experiments reported here. For example, is chl and primary production simulated in exp 1yS more realistic than in the other scenarios investigated?. Without such (at least qualitatively) comparison the reader remains uncertain about the robustness of the conclusions.

The referee is right that it could be useful to show, and we will add therefore add a validation similar to the one in Fransner et al. 2018 in the supplementary material. The differences between the experiments presented here and the one in Fransner et al 2018 are minor for the monitoring stations located in the middle of the basins. The largest differences are found in the coastal areas (which is why we show figure S4 in the supplementary material). At these stations there are not enough measurements of chlorophyll to make a validation of the model, which is why we only show DIN and DIP.

8) There is no mention of tDOM stoichiometry. How do DON and DOP discharged by the rivers affect primary production in the investigated area? Is the simulated primary production more realistic when riverine discharge was considered in the model? This question could be answered by comparing the model experiment with tDOM with the experiment without tDOM.

In all experiments there is a release of terrestrial organic nutrients (DOM and DOP) from the rivers. Its release and degradation is kept constant over all experiment to make sure that differences in pCO2 are not caused by changes in primary production. This is explained at line 20 in the manuscript. Earlier studies and sensitivity experiments have shown that organic nutrients are important nutrient sources for phytoplankton in the Baltic Sea. We will add some text to make this clearer.

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References:

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