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Discussions

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

Anonymous Referee #1

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The manuscript describes a modeling exercise meant to investigate the fate of terrigenous DOC (tDOC) in the Northern Baltic Sea. More specifically, the authors provide an explanation to the high pCO2 observed in the area, concluding that this is due to the remineralization of tDOC by bacteria and the concomitant reduction in productivity due to the absorption of light by tDOC. The topic investigated is very interesting and the findings are relatively novel as the main removal process of tDOC was often assumed to be photo-degradation. The fate of the large amount of tDOC discharged into the ocean is still an open question in coastal oceanography and the results of this paper provide novel insights. The manuscript is clear and well written, the succession and explanation of the experiments are clear and logic. I would recommend publication

after the authors have considered/discussed/clarified the following points:

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. 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. 3) tDOC is given in ïĄ■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? 4) Why Kdtdoc is not equal to 0 when tDOC is zero? 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. 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, L&O) 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 production 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. 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

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