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Discussions

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*Interactive comment on* "Carbon cycling on the East Siberian Arctic Shelf – a change in air-sea CO$_2$ flux induced by mineralization of terrestrial organic carbon" *by* Erik Gustafsson et al.

**Anonymous Referee #1**

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The presented manuscript aims to use field collected observations of carbon and carbon isotope tracers to constrain a regional circulation-carbon cycling model of the East Siberian Arctic Shelf. The goal is be able to scale up the regional fluxes of terrestrial organic carbon delivery and removal as well as the effect on air-sea CO2 exchange for the region. While the approach is justified, the current execution of the model-data fusion and its output are insufficient to answer the scientific questions posed by the authors. I recommend further refinement of their methods by expanding to more regional boxes in their model to better capture the variability observed in the data.

General Comments:

Appendix A: It's not clear how the boundary conditions were generated. The model

has 3 boxes in the x and y but how many in z? Are there additional boundary x/y/z boxes with computed physics?

Appendix B: The text is vague as to the specifics for how many of the biogeochemical processes are handled in the model. For example, how are the three autotrophs different in their growth, mortality, production of organic matter tracers etc.? How is the temperature dependent remineralization of organic matter modeled, with an Arrhenius type formulation? What fraction of POC reaching the sediments is respired back to the bottom of the water column vs. buried? What is the fraction of and rate of POC solubilization to DOC? At what oxygen concentration does nitrate reduction take over to oxidize organic matter? Many of these concerns could be alleviated by including the model equations for the state variables with the parameter values in a table. Also is POC advected between boxes or not? As written I'm under the impression that it is, which is contrary to most physical-biogeochemical models, which consider POC to sink only in the vertical below where it is produced/added. Or is the POC in this model of small enough size that it doesn't sink and instead can be advected with the circulation?

Table B1 and B2: Shouldn't the units for all 13C carbon quantities be ‰

Appendix C: This section provides little evidence for how 13C is cycled between the carbon tracers in the model. Again this could be improved by adding equations and/or a table detailing the prescribed $\delta$13C values for certain C pools or the isotopic fractionation factors for the various processes, e.g. air-sea gas exchange, DIC assimilation, POC solubilization, etc.

Pg5 L36: The model is 1D? The description in Appendix A describes a 3D model with 3 boxes in the x/y and some unknown number of vertical levels. Which is it?

Fig 3: Is there no $\delta$13C-DOC data for the Inner Laptev Sea? No observation symbols appear on its plot. Much of the data for a number of tracers exhibit a large range in variability for a given region (ILS, OLS, ESS). For example, DOC varies by a factor of 4, $\delta$13C-DIC by a factor of 3 for a given depth layer. This leads me to question the validity

of the method employed, i.e. large-scale horizontal integration of the ILS, OLS, ESS regions. In other words, I think it is too large of a simplification of the system to model the ESAS mixing and biogeochemistry with only 3 horizontal boxes. This is why none of the model outputs from the sensitivity suite can do an adequate job of matching the observations for a number of tracers. This is most apparent for ESS DOC, OLS $\delta$13C-DOC, ESS $\delta$13C-DOC, and all three regions for $\delta$13C-DIC.

Fig 4: My same concern with respect to the number of horizontal model boxes arises here too. It is readily apparent that the model fails to capture the variability in all plotted tracers: AOU, NO3, PO4, and fCO2. A goal of the study is to quantify the effect of terrigenous OC mineralization on fCO2 and thus air-sea CO2 exchange yet none of the model suite can reproduce the plus or minus $\sim$30% variability of fCO2 observed at the surface.

Section 3.2 & Table 2/3: What are the uncertainties on all of these fluxes mentioned in the text and within the tables? Also there is no attempt by the authors to formally quantify how well or poorly each model tested can reproduce the data. E.g. calculation of the mean bias between model simulated and observed tracers, correlation coefficient, relative amount of the variance captured, etc. There is most likely a family of parameter values that fit the data to within some computed metric that could be used to estimate the uncertainty in the reported fluxes.

Figures 6/7 and Pg9 L11-26 should be moved to Section 3 Results.

Pg 9 L41-42 & Pg 10 L1-2: Many have suggested flocculation and sedimentation near river mouths to be an important sink for terrigenous DOC in the Arctic (Sholkovitz et al., 1978 EPSL 40; Droppo et al., 1998 Arctic 51; Cauwet, 2002 Biogeochemistry of marine dissolved organic matter textbook and references therein). Can the authors provide further evidence or make a stronger case for why their model results suggest it is not? This important point deserves more detail other than to say including it doesn't fit the data well.

Pg 10 L7-11: If 100% labile riverine POCter is unrealistic and unconstrained then why not describe the sensitivity of the resulting C fluxes to differing assumptions in its lability? Are the model results very sensitive to this unconstrained parameter or not?

Pg 10 L18 & L37-43: Why was a C:N:P of 106:8:1 specifically tested and not other C:N:P stoichiometries? Are there prior studies indicating that this lower N demand stoichiometry better describes phytoplankton production in this region vs. Redfield stoichiometry? It seems that one of the main conclusions of the study (whether the ESAS is a net sink or source of atmospheric CO2) is very sensitive to the choice of phytoplankton stoichiometry!

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