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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 #2

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The authors present a physical-biogeochemal model of carbon cycle processes in the East Siberian Arctic Shelf (ESAS). Observational data from two oceanographic surveys are combined with World Ocean Data and published literature on the terrestrial carbon supply from the major ESAS rivers (Lena and Kolyma) to validate the model, with the main goal of determining the pathways and processing of terrestrial carbon supply to the ESAS shelf. Although this model strives to be extremely comprehensive in its parameterizations of the physical and biogeochemical coupling within the ESAS, I found many important details were lacking as to how the model treats these intertwining components, and, in particular, the sensitivity of the model results to variability in the initial conditions. I'm concerned that the model poorly reproduces vertical profile

observations of carbon parameters and misses the surface ocean observations altogether, much reducing any confidence in the conclusions of the paper. If the model is still able to reproduce the same results after taking into consideration all the discussion/reviewer comments, I think this analyses has the potential to generate some very interesting, testable hypothesises for future observational studies in this region. In particular, the current model results highlight the importance of phytoplankton CO2 assimilation, carbon burial, and export to the Arctic Ocean in compensating for the impact of OC degradation within the ESAS; this insight is extremely informative and can be used as a baseline to consider the impact of future change in this region. Likewise, the current model result indicating the importance of the supply of eroded labile terrestrial POC and coastline erosion in controlling CO2 emissions (over that of changes to river DOC flux) can be used as a basis for comparative studies in other Arctic delta systems.

Specific Comments:

Section 1, Tables B1 & B2: Please indicate what stable isotope end members are used to distinguish marine and terrestrial C components in this model. Paragraphs 2&3 on page 2 seem to give some examples of various end members that could be applied, but are these the ones actually used in the model? A table clearly defining the end members and their uncertainties is necessary, in particular, the overlap of 13C end members for marine and terrestrial POC and what influence this has on the model output should be discussed.

Model constraints (Section 2.1 + Figure S2): The model outlined in Figure S2 concerns only 1 dimension and 1 flow direction (e.g., flow of water from Box II into Box III then out of the system; flow from Box I into Box II into Box III then out of the system) is there no opportunity for lateral exchanges across the boundaries? (e.g., flow from outside INTO Box III? as described in page 3, line 17-18) Some discussion is needed to indicate what proportion of C cycling might be neglected by omitting exchanges from the shelf and open ocean into the boxes (or revers exchanges between boxes).

For example, what about importing marine DIC and DOC from either the Atlantic or Pacific exchanges along the model boundaries? What impact would these exchanges have on the OCmar in your model?

Data to inform model (section 2.3.2): what is the variability of stable isotope values for each of the inputs and how does this influence the model output? In the model terrestrial end members are at constant values instead of accounting for a temporally variable input; for example, river 13C-POC as -29 permil, however the standard deviation in the PARTNERS timeseries data set for the Lena River alone is +/-2.7 permil, overlapping values used for eroded OCter and likely marine POC (not listed). Some sensitivity study or discussion is warranted to indicate how sensitive your model is to changing the end member stable isotope composition of the C inputs.

Model results (Section 3.2 + 3.3): I'm concerned that the model isn't able to reproduce surface ocean observations (in particular within the Lena delta), yet the authors seem quite sure of the determination of CO2 source/sink status for different boxes. I would like to see the model better reproduce some of the surface structure of the observations before I'd feel comfortable estimating air-sea fluxes of CO2.

Summary + Concluding Remarks (Section 5): "DOCter supplied by rivers appears to be largely refractory" (page 11, line 20), does this mean that the material is indeed refractory or that it is buried before it can be mineralized? The model might not be sensitive to this difference, but the system may well be.

Summary + Concluding Remarks (Section 5): bullet 2 (line 22), what about the importance of imported DIC and DOC (or even POC) from the Atlantic and Pacific? (as mentioned above)

Technical Corrections:

- literature citations should be added to describe the importance of permafrost thaw and coastal erosion in the ESAS (page 1 line 33/34); a polar reference would be preferred

instead of the reference to Deutsch et al 2012 (page 2 line 8; for example, Sallon et al Polar Biol (2011) 34:1989–2005); and, some other published literature citation should be used instead of the cruise report reference describing 13C-CO2 gradients in the ESAS (page 2 line 18/19)

- the use of "e.g." throughout the text is generally redundant and should be removed at the following instances: page 2: lines 18, 21, 28, page 3: lines 5, 27, page 8: line 33

- please check the order of literature cited (references), for example: Cavalieri et al comes between Dee et al and Deutsch et al

- Tables B1 & B2, should 13C units not be permil? (‰

- Figure 1: please add both rivers to your map (Lena + Kolyma)

- Figure 6: can your observations be added to these simulated seasonal values? How closely are they reproduced?

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