Reply to RC1 (Responses in *italics*)

General comment

This manuscript aims at applying a model that describes dissolved organic carbon dynamics at the Changjiang Estuary, in order to identify the different processes affecting DOC dynamics in this particular site.

In general the manuscript is well written and easy to follow in all its parts. Although I am not an expert on modeling, I was able to follow the description and the application of the model that the authors used. I cannot comment on the specifics on the model used, as I am not an expert in this field, but it was clear enough that the model was properly validated by observational data over a large time scale.

In my opinion however, the authors need to work on the discussion part of the manuscript to highlight the importance of this study. Some important questions need to be answered, such as: why is this model important? what new information it gives that improves the knowledge on this region? how these results/model can be of use in other regions?

The discussion needs therefore to be re written taking these questions in mind. In the current form the discussion contains mostly a literature review on previous studies, not properly compared to the results of this study.

Also the introduction strongly needs a revision in order to have an updated bibliography. There are too many outdated references, sometimes as the only ones, that can be updated with much more recent studies.

Many thanks to the reviewer for the helpful suggestions.

The discussion section will be expanded to emphasize the significance of our study. We will refer to the open research question, summarize key findings of our work to address the question, and compare our findings with existing literature to highlight the significance and implications of our study for the field.

We will also improve the Introduction and refer to more recent studies. Our specific responses to the comments are listed below.

Specific comments

Line 15: salinity has no units. To be checked throughout the entire manuscript.

We will add the salinity unit (PSU) and check throughout the manuscript.

Lines 23-24: “DOC is defined as the nonliving organic matter in the ocean that can pass a 0.5µm pore size filter…”. This definition is not correct. DOC is operationally defined as the organic carbon that passes through a 0.2 µm pore size filter. Sometimes also 0.77 µm pore size filter is used. Also the references here needs to be adjourned.

We will rephrase the definition of DOC to make it more precise and accurate and updated the references: “DOC is typically defined as the fraction of organic carbon in the ocean that can pass a pore size filter ranging from 0.2-0.77µm, with decomposition time scales varying from hours to years (Asmala et al., 2014; Carlson & Hansell, 2015; He et al., 2016)”.

Lines 27-28: "DOC in estuaries is predominantly derived from river inputs and marine production". In situ production processes should be mentioned too here. Also because they are mentioned later in the manuscript.

This sentence will be rephrased as: "*DOC in estuaries is predominantly derived from river inputs and marine in situ production*".

Line 78: SPM needs to be defined.

Definition of SPM will be added.

Lined 89-92: Here the authors introduce DOM, however in the manuscript only DOC is discussed. Either the authors mention only DOC or it should be clarified the link between DOC and DOM.

Indeed, we will add a description of the relationship between DOC and DOM in the manuscript: "DOC represents the carbon fraction within dissolved organic matter (DOM) (Hopkinson & Vallino, 2005)".

Line 96: Why the turnover time scales taken into account are limited to 70 years. Refractory DOC has a turnover time that is much larger than 70 years. For turnover time of DOC see Hansell, 2013 (https://doi.org/10.1146/annurev-marine-120710-100757).

Following classification and nomenclature of DOC by Hansell (2013), the biogeochemical model (ERSEM) used in this study only considers DOC with a maximum lifetime up to 70 years. This means that refractory DOC with turnover time scale >70 years is assumed to have the same turnover rate as the semi-refractory DOS in the budget calculation in our study. We believe that the potential error is minor and does not affect the overall ratio of source and sink terms in the budget analysis since our model simulation covers only a period of 5 years, which is much shorter than the turnover time of semi-refractory DOC. A justification as well as a discussion of the potential error in the model configuration for refractory DOC will be provided in the revised manuscript.

Line 145: Table 1. Some data miss the data source. Where are these data coming from?

The missing source in the table was due to data from different time periods being derived from the same reference, hence the cells in the table were merged. We will optimize the table to avoid this misunderstanding.

Line 159: Reference to figure S1. There should be also figure S2. These two figures are useful to understand the comparison between the observational data and the model data. However being two different figures and being the images (surface/bottom, summer/winter) displaced in a different order a comparison is difficult to make. I would suggest to merge these two figure together and displace the observational/modeled data next to each other.

By looking at these figures, I think it would be interesting to also highlight where there is least correspondence between the observations and the model and discuss that too.

Thanks for the suggestion for merging Figures S1 and S2 for a better comparison. We will merge these two figures and rearrange the figures.

We will add discussion regarding the least correspondence between the observations and the *model. The mismatch mainly occurs in the north-eastward expansion of the elevated DOC concentration in the surface layer during summer. This inconsistency may be attributed to the uncertainties in both measurement and modelling: including the uncertainty in sampling time (most of the sampling data only covered a few days within a month), locations (some points had more data collected, while others had less), limited availability of collected data. The inconsistency may also be due to the limitation of model to capture the driving forcing for the north-eastward diffusion of DOC during summer. Nevertheless, a general agreement in the DOC concentration between model and observational data is shown. We will further investigate the mismatch for the north-eastward expansion of the elevated DOC concentration in the surface layer during summer between the model results and observation data and add uncertainty estimate due to this mismatch.*

Lines 167-168: "the seasonal average of the model results over the years 2013-2017 was calculated for all four seasons". Why only summer and winter were chosen?

The focus on summer and winter in our interpretation is primarily due to the following reasons:

Most observational data (Table 1) were collected during summer and winter, allowing for a corresponding and extended analysis and discussion based on the observational studies.

Additionally, these two seasons exhibit significant and representative differences in hydrological and ecological characteristics. Summer is marked by the flood season of the Changjiang Estuary, whereas winter is the dry season. The prevailing wind directions also differ between these seasons, leading to notable variations in hydrodynamics. Moreover, the higher temperature, strong stratification and abundant nutrients in summer enhance ecosystem activity and primary productivity, while the colder temperatures and stronger vertical mixing in winter reduce ecological activity. These factors directly impact the DOC cycling in the estuary. Therefore, discussing the source-to-sink pathways of DOC in terms of summer and winter makes the study more representative and concise. The other two seasons, namely spring and autumn, act as transition periods between the two distinct regimes in terms of hydrological and ecological characteristics and therefore not the focus in the main text descriptions.

We will add explanations on why summer and winter seasons were chosen for detailed interpretations in the main text.

Line 181: In the caption of Figure 4 there is no details on the 6 panels. Also it is not specified if these plots are from observational or model data.

Thanks for pointing this out. The caption of this figure will be improved accordingly.

Line 242: "The released mDOC from bacteria is converted into semi-refractory mDOC". This statement needs to be supported by a reference.

The following explanations with reference will be added for clarification.

" The ecosystem model ERSEM includes the bacteria-mediated production of recalcitrant DOC (Hansell, 2013), which represents the most difficult-to-digest semi-refractory DOC in the model. Therefore, the newly produced mDOC from bateria is added to the pool of semi-refractory mDOC, thereby an implementation of the microbial carbon pump (Jiao & Azam, 2011; Jiao et al., 2014)."

Line 250: Figure 5 is difficult to read, especially the dotted lines, which are too similar as color and too thin

We will improve the image and optimize the colours to enhance visibility.

References in our response which will be added in a revised manuscript

Asmala, E., Bowers, D. G., Autio, R., Kaartokallio, H., & Thomas, D. N. (2014). Qualitative changes of riverine dissolved organic matter at low salinities due to flocculation. *Journal of Geophysical Research: Biogeosciences*, *119*(10), 1919-1933. https://doi.org/10.1002/2014jg002722.

Carlson, C. A., & Hansell, D. A. (2015). *DOM Sources, Sinks, Reactivity, and Budgets*. https://doi.org/10.1016/b978-0-12-405940-5.00003-0

Hansell, D. A. (2013). Recalcitrant Dissolved Organic Carbon Fractions. In C. A. Carlson & S. J. Giovannoni (Eds.), *Annual Review of Marine Science, Vol 5* (Vol. 5, pp. 421-445). https://doi.org/10.1146/annurev-marine-120710-100757

He, W., Chen, M., Schlautman, M. A., & Hur, J. (2016). Dynamic exchanges between DOM and POM pools in coastal and inland aquatic ecosystems: A review. *Sci Total Environ*, *551-552*, 415-428. https://doi.org/10.1016/j.scitotenv.2016.02.031.

Hopkinson, C. S., & Vallino, J. J. (2005). Efficient export of carbon to the deep ocean through dissolved organic matter. *Nature*, *433*(7022), 142-145. https://doi.org/10.1038/nature03191.

Jiao, N., & Azam, F. (2011). Microbial carbon pump and its significance for carbon sequestration in the ocean. *Microbial Carbon Pump in the Ocean*, *10*, 43-45.

Jiao, N., Robinson, C., Azam, F., Thomas, H., Baltar, F., Dang, H., . . . Zhang, R. (2014). Mechanisms of microbial carbon sequestration in the ocean – future research directions. *Biogeosciences*, *11*(19), 5285-5306. https://doi.org/10.5194/bg-11-5285-2014.