

Interactive comment on “Modeling inorganic carbon dynamics in the Seine River continuum in France” by Audrey Marescaux et al.

Anonymous Referee #1

Received and published: 13 January 2020

This paper described a biogeochemical model incorporating inorganic carbon cycle and applied the model to the Seine River system. The model was built based on an existing biogeochemical model and the model structure and setup have been sufficiently described. The results from current study help to fill up the gaps in understanding the contribution of inland waters to the global carbon cycle. However, the model performance is not very convincing. There had been a few other models able to simulate inorganic carbon in rivers and have not been discussed. In summary, the manuscript has potential to be improved and I would like to suggest the authors to consider:

1. improving the model performance presentation (see specific comments below);
2. the discussion of current findings is too site-specific; I would suggest to expand the discussion to a more general sense, e.g. how the inorganic carbon system in Seine

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compared to other inland water systems? What are the meaning of current findings to estimating the roles of rivers in local and global carbon cycle? etc.

3. Also, the text writing in the introduction and discussion need to be polished. I list a few issues in the specific comments below but encourage the authors to go through the text and improve the writing in general.

Specific comments:

Line 37-38: some words are missing from this sentence. 'Outgassing was the most important {carbon sink/inorganic carbon process?}'

Line 69-71: This statement seems controversial to some other findings that eutrophic system usually contains richer organic matters and pCO₂ (e.g. Borges and Abril, carbon dioxide and methane dynamics in estuaries, DOI: 10.1016/B978-0-12-374711-2.00504-0). Can you please explain more about this statement?

Line 69-71: The Xu et al. 2019 reference is missing;

Line 72-76: This statement needs to be treated carefully. Other methods, such isotope surveys, can also be used to investigate the fate of carbon in aquatic systems.

Line 85-90: A few early papers had reported models including the inorganic carbon cycle and pCO₂ exchange but have not been mentioned here. Such as the CONTRASTE model (Vanderborght et al 2002, Application of a transport-reaction model to the estimation of biogas fluxes in the Scheldt estuary, Biogeochemistry 59: 207-237), RTM model (Regnier et al 2013, modelling estuarine biogeochemical dynamics: from the local to the global scale, Aquat Geochem 19: 591-626); How is the current model compared to these models?

Line 91-92: This individual sentence as one paragraph is not reading well. Can be merged with next paragraphs.

Line 111: unit of the north and east coordinates?

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Line 228-230: the gas transfer velocity only affect the exchange rate, not the change direction of pCO₂ (and therefore DIC).

Line 383-384: why only 4 years simulated but NRMSE were performed on inter-annual variations per decade, instead of 2010-2013? Also, normalized against mean observational data instead of inter-annual variations is more representative.

Line 402-404: as CO₂ concentrations are related to DIC and TA, it would be better if you show the comparisons of observed and modelled DIC/TA along with the CO₂ concentrations.

Line 517-518: can't find CO₂ outgassing in figure 9?

Figure 6: why there are two dark lines in the water flow of the outlet of the basin? Also, as the model timeframe includes dry and wet years, it is better to show the results year to year but not averaged from 4 simulated years;

Line 583-589: this sentence needs to be re-organized.

Line 624: left bracket is missing in citation;

Section 4.3: is there a relationship between the river eutrophic state and the metabolism activity, and CO₂ outgassing?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-601>, 2019.

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