Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC1
https://doi.org/10.5194/bg-2021-209-RC1, 2021

Comment on bg-2021-209

Anonymous Referee #1

Referee comment on "Carbon dynamics at the river–estuarine transition: a comparison among tributaries of Chesapeake Bay" by Paul A. Bukaveckas, Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-209-RC1, 2021

General Comments

The author presents a study in which he uses mass balance and ecosystem metabolism data to generate carbon (C) sources and transformations in the James, Mattaponi, and Pamunkey River Estuaries. He found that the C inputs differed between rivers and season based on watershed characteristics and discharge. Contrary to his prediction, highest retention of organic C occurred during periods of relatively high discharge. These systems were net heterotrophic, though there was some contribution from autotrophy that varied by river and season. Finally, the author applied a bioenergetics model to estimate the proportion of organic C removed by catfish, bald eagles, and osprey.

This is a nice study that will be well received by the readers of this journal. The study is a thorough examination of the C cycle in terms of external (river inputs, tidal exchange) versus internal (metabolism) drivers in influencing the forms and fluxes of C in the study systems. The manuscript is well written, clear, and well organized, and I think this is a very strong and interesting dataset. For the James River, they have a relatively complete, impressive C budget dataset that spans 10 years. For the Mattaponi and Pamunkey Rivers, the dataset is less complete and spans only 2 years. But the systems are different enough that it is worth including the analysis of the less-sampled Mattaponi and Pamunkey Rivers for comparison. There are a lot of display items (2 tables + 11 figures + supplemental material), but they all seem to serve a purpose, so I don’t recommend dropping any. Overall, I am comfortable with the conclusions and support publication of this manuscript with minor edits, as detailed below.

Specific Comments

Lines 70-89 Other studies to consider for this section of tidal freshwater zones:

Xu, X., H. Wei, T. Light, S. Melton, K. Holt, G. Barker, A. Salamanca, B. Hodges, K. Moffett, J. McClelland, A.K. Hardison. 2021. Tidal freshwater zones as hotspots for biogeochemical cycling. *Estuaries and Coasts* 44:722-733. DOI: 10.1007/s12237-020-00791-4.

Jones, A.E., A.K. Hardison, B. R. Hodges, J.W. McClelland, K. B. Moffett. 2019. An expanded rating curve model to estimate river discharge during tidal influences across the progressive-mixed-standing wave spectrum. *PLoS ONE* 14(12):e0225758*,* doi:10.1371/journal.pone.0225758.

Jones, A.E., B.R. Hodges, J.W. McClelland, A.K. Hardison, and K.B. Moffett. 2017. Residence time-based classification of surface water systems. *Water Resources Research*, 53:5567-5584, doi:10.1002/2016WR019928.

Line 82 and elsewhere Is there a reason why you refer to your systems as the James Estuary and not the James River Estuary? (Similar for the Mattaponi and Pamunkey River Estuaries)

Line 157 How did you determine the “constant fraction” the ungauged discharge was relative to the Fall Line discharge?

Line 214 Define GPP and ER abbreviations.

Line 234 Define PQ and RQ abbreviations.

Line 312-325 Refer more often to Fig. 4 and Table 2 throughout this text. (Also, please do this in the subsequent paragraphs explaining Figs. 5, 6.)

Line 435 Replace “reveled” with “revealed”

Lines 438-440 Explain briefly which rocks in the Mountain and Piedmont regions contribute substantially to DIC runoff.

Line 446 and elsewhere Since you are the sole author of this manuscript, you may not want to use the “we” pronoun.

Lines 484-487 Your findings suggest the inland waters function as pipes during high discharge periods. This is counterintuitive, as one would expect particulates to not be able to settle during high discharge relative to lower discharge. Can you expand on this concept? Are your data an exception to a relatively well-established rule established from other systems? What mechanism in your system might be at play?

Lines 490-500 Your data suggested use of a lower exchange coefficient (1 to 1.5 m/d; section 2.7), and you ended up using a value ~4x higher (4.3 m/d) based on values published by Raymond and colleagues. But in this section of the discussion, you refer to another study where Raymond used a value closer to the low value (1.1 m/d), so you then suggested that might be more appropriate to get your values closer to the Raymond et al. 2000 air-water fluxes. It seems to me like you should have stuck with your data-driven value (1 to 1.5 m/d) in the first place? This issue warrants further explanation in the methods and discussion.

Lines 533-535 What characteristics of the Susquehanna River and Chesapeake Bay mainstem make them net autotrophic?

Line 548 Insert “times” after “residence”

Line 584 Insert “of” before “POC”