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Interactive comment on "Dissolved carbon biogeochemistry and export in mangrove-dominated rivers of the Florida Everglades" *by* David T. Ho et al.

Anonymous Referee #2

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Major comments

Mangroves play an important role in carbon cycling on local, regional, and perhaps global scales, and yet there is a lack of detailed budget studies. This paper addresses that need. The suite of measurementsâĂŤincluding DIC, DOC, carbon isotopes, oxygen, and alkalinityâĂŤis appropriate for the task of determining the sources and sinks of organic and inorganic carbon involving a variety of processes. The measurements show very clear non-conservative behavior in the two tidal rivers studied, strongly suggesting additions of DIC and DOC as water transits through these estuaries.

This paper presents a high-quality suite of measurements from experienced aquatic biogeochemists. The main problem is that the method of creating a carbon budget for the two estuaries is very confusing. Because of this, it is difficult for me to evaluate the main conclusions of the paper. Therefore, I suggest major revisions.

The technique by which the authors quantify gas fluxes is clear and robust, though error estimates would be helpful. It's the longitudinal fluxesâĂŤas the authors call themâĂŤthat cause confusion. As the authors use the term, the longitudinal flux is averaged over the time period of the tracer release and hence accounts for tidal dispersion as well as the net downstream advective transport. The problem is that the authors use the term too broadly and in ways that defy notions of mass conservation. I specifically take issue with a longitudinal flux due to a certain source, such as freshwater wetlands, mangroves, calcium carbonate, or the ocean (lines 18-21); or due to a certain process, such as estuarine or non-estuarine (lines 289-290 and Table 4), and respiration or dissolution (lines 217-218). A longitudinal flux is due to movement of water and nothing else. It's a term in the mass conservation equation that is separate from internal sources and sinks. The two should not be mixed up.

Also confusing is the alternation between approaches based on inventories and approaches based on fluxes. My suggestion is to focus on a budget approach because this paper is basically about the carbon balance of two estuaries. The final budget is shown in a key figure towards the end of the paper (Figure 5), which clearly shows the control volume approach I am recommending. Remarkably, there is not a single budget equation in the paper. And the method for determining the freshwater wetlands flux shown in this figure should be detailed in the methods, not the figure caption. In the paper, the authors should start with a budget equation and then use inventory equations, such as Equations 2 and 9, as needed to support the budget approach. Budget equations should be shown for DOC, DIC, oxygen, and (perhaps) alkalinity.

A process that appears to be missing in the paper is organic mineralization inside the estuary. The very low DO suggests quite significant net heterotrophy. The authors lump organic mineralization inside the estuary with mangrove root respiration and call it a mangrove source of DIC. Those seem like two very different sources to me, which could

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perhaps be distinguished using the isotopes. Figure 5 is remarkable in that it does not show any in situ production or consumption. The net organic matter remineralization should be revealed in the DO budget of the individual terms. The "Evidence from DO" section (3.3.3) should basically detail the DO budget: gas exchange, upstream input, downstream export, net in situ consumption, etc. And this budget could be nicely shown in an additional panel on Figure 5.

Photosynthesis in the river is ignored, which may be reasonable, but the authors need to be more convincing (lines 228-231). Saying chlorophyll is low is not enough. They state that diurnal pCO2 variations are small but provide no data to support that. Please be quantitative.

The choice of endmembers is not clear. How exactly are these chosen and what are the values?

The use of X +/- Y throughout the text (lines 192, 299, 319, 333, 343, 375, etc.) and tables (1, 2, and 3) is unclear. How are X and Y computed? Am I supposed to take Y as a standard deviation or a standard error? What is the sample size for computing X and Y.

Minor comments

Line 25: I think the area referred to here in this flux is the forest area, but the authors should make it clear to remove any ambiguity.

Line 26: To be clear that you are not talking about the Everglades in general, I would replace "in this region" with "for the Shark and Harney Rivers"

I was pleased to see that the authors recognize the likelihood of large seasonality in the carbon cycle of south Florida estuaries (Section 3.7). However, the authors give no indication as to why they chose to sample in November two years in a row instead of sampling once in the wet season and once in the dry season, which would have given them a sense of the importance of seasonality. All I am asking for here is a

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few sentences in the intro or methods describing the rationale for the sampling periods chosen.

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