Major comments

The technique by which the authors quantify gas fluxes is clear and robust, though error estimates would be helpful. It's the longitudinal fluxes, as the authors call them, that 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.

It is true that longitudinal water flux would be due solely to movement of water. However, longitudinal carbon flux is due to movement of water and the concentration of dissolved carbon in the water. When we talk about contributions, we are not talking about the contribution of the freshwater marsh, mangroves, or carbonate to the water movement, but to the dissolved carbon concentration.

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.

We determined the inventories of DIC and DOC in order to calculate the fluxes. Our ultimate goal is to quantify the aquatic flux of mangrove-derived carbon based on: Flux = Inventory/Residence Time, where residence times were determined from the $^3He/SF_6$ tracer release experiments. To determine mangrove derived DIC and DOC fluxes, we needed to find out how much DIC and DOC were in the rivers, and also the sources of this carbon. Then, we need to determine the fate of the mangrove-derived carbon in the river (i.e., lost to the atmosphere or to the coastal ocean).

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 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.

We consider all organic matter added in the estuary as mangrove derived, so the reviewer is correct that we are consider DIC produced from organic mineralization inside the estuary as mangrove derived CO_2. In the final budget, it does not matter whether the mineralization occurred in the mangrove sediments, or in the river.

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 pCO_2 variations are small but provide no data to support that. Please be quantitative.

As we state in the manuscript, photosynthesis in the river is ignored because of low chl *a* and low phytoplankton biomass. With respect to day/night pCO_2 variations, we make continuous hourly measurements of pCO_2 from Shark River. The average difference in pCO_2 during the night (6p to 5:59a local time) is 3% lower than during the day (6a to 5:59p local time), a small amount and not in the direction one might expect if photosynthesis were significant. We have added this quantification to the manuscript.

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

We write in the manuscript that "the freshwater and marine end-members were assigned to the values measured at the lowest (Tarpon Bay) and highest salinities, respectively." Basically, we went as far as up river and out to the Gulf of Mexico as we could with the boat that we had, and we chose those measurements as the end-members. As we state in the paper, "During SharkTREx 1, the salinity along the longitudinal transects ranged from 1.2 to 27.1, and the mean (± s.d.) water temperature was 23.4 ± 0.2 °C. During SharkTREx 2, salinity ranged from 0.6 to 27.1, and water temperatures averaged 22.7 ± 0.9 °C."

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.

Depending on the parameter, the error estimates are based on standard deviations of the measured parameters, or the propagated errors of variables used to calculate the parameter. We have indicated this in the appropriate places in the manuscript.

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.

Yes, we have added language that indicates that flux is from the forest.

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"

Done

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

There was no good scientific reason for choosing to conduct the experiments in November, twice. SharkTREx 1 and 2 were piggy-backed on funded experiments that took the team from Hawaii to Florida, and these funded experiments had to be conducted in November. SharkTREx 1 was a pilot experiment, and focused on just the Shark River, had fewer stations, and where some important measurements were not made (e.g., DIC). SharkTREx 2 was a follow up to fill some of the gaps (i.e., more stations, Harney River, and high quality DIC measurements). Future studies will be conducted in the dry season.