We appreciate Reviewer 1's thorough comments and constructive criticism of our manuscript. Their primary critique is well-founded and articulated, and we are thankful for the detailed argument that they have laid out in their review. Indeed, flow in seagrass systems is complex, producing vertical heterogeneities in water column physical and chemical properties. For example, flow is significantly reduced in the canopy, increasing the residence time of water in the canopy relative to the overlying water (Peterson et al, 2004). Shear between these two compartments (in and out of the canopy) drives vertical exchange across the canopy interface that partially or wholly homogenizes water chemistry. At a smaller scale, this turbulent mixing also helps to alleviate carbon limitation that may build up in the seagrass blade boundary layer (Koch 1994). Our NEP/NEC estimates were derived from concentrations measured near the surface. These measurements represent the cumulative effect of lateral DIC/TA fluxes (which we assume to be minor) and turbulent/diffusive exchange between the seagrass canopy and overlying water (which we assume are dominant). This is a partial motivation for why we chose surface-water rather than within-canopy measurements, because it integrates the seagrass metabolic signal from a larger footprint. Still, there is a potential for our slack water approach to be biased by lateral water exchanges, which we will try to address to the best of our ability here.

Unfortunately, we don't have any empirical data specifically addressing the spatial variability of carbonate chemistry at these sites, but we can build one line of evidence from the data that we do have. Our two sites are separated by a linear distance of approximately 4 km. Looking at figure 2, we can approximate the difference in nTA and nDIC between the sites to be at most 300 μmol/kg. Hence, we have an approximate spatial gradient of at most 75 μmol/kg/km (300 μmol/kg / 4km). This corresponds to at most 7.5 μmol/kg over a 100m stretch, which is about half of the 13 μmol/kg estimate that reviewer 1 derives in their comments. Furthermore, our seagrass meadows are much larger than 100m, in fact are typically a factor of ~5x greater (>0.5 km^2). Hence, the comparable TA gradient required to explain our metabolic fluxes would be appreciably greater, on the order of ~65 μmol/kg.

As further evidence, we are including the following figure which shows current speed and direction from the tilt current meters (TCMs). From this, it appears that flow was not unidirectional at these sites over the study period, but was instead variable in direction without a clear mode which might suggest tidal or wind-seiche. While we are reluctant to use these data in our manuscript because the water velocities were below the detection limit of the TCM, we hope they offer some support to our argument in this discussion forum. One prior study at a site just west of ours also reported generally low water current, especially within the seagrass canopy, despite a slightly greater tidal influence there (Hansen et al., 2017). Hence, we feel confident that water current at our site was indeed low. We also see no clear link between current direction and changes in TA/DIC, which should be apparent if there was a distinct TA/DIC source whose signature was being advected over our site. For example, there was a subtle decrease in salinity of ~0.5 on the morning of 11/27 at the high-density site (Fig. 2), but the indicated current speed and direction were apparently consistent during this time period (attached figure). Lastly, it is possible that small inputs of fresh water, either through surface or groundwater channels may have significant and nonlinear impacts on carbonate chemistry. However, the attached scatter plot of salinity vs depth indicates that the small changes in water level we observed did not coincide with any clear changes in salinity (i.e. freshwater input).

So, we strongly agree that the combination of spatial variability in carbonate chemistry and advection can cause TA/DIC variability that may impact the ability to estimate NEM/NEC. This would be especially problematic if we had collected water samples *within* the seagrass canopy where water chemistry is much more variable in space/time. However, if we consider all of these lines of evidence, along with the fact that our measurements were made above the canopy, we argue that lateral mixing over the study period was likely relatively low, and likely not sufficient to drive the diel variations we observed, which were generally 50-100 μmol/kg.

In light of Reviewer 1's concerns regarding the assumptions involved in the TA/DIC budget used for Figure 9, we have elected to remove section 4.3 (TA/DIC export) and figure 9 from the manuscript. Furthermore, we have made a concerted effort to more clearly state the assumptions and limitations of our 'slack water' approach throughout the manuscript.

Peterson, C & Luettich, Jr, R & Micheli, F & Skilleter, G. (2004). Attenuation of water flow inside seagrass canopies of differing structure. Marine Ecology Progress Series. 268.

Hansen, J. C. R., & Reidenbach, M. A. (2017). Turbulent mixing and fluid transport within Florida Bay seagrass meadows. Advances in Water Resources, 108, 205–215. doi:10.1016/j.advwatres.2017.08.001

Koch, E.W. Marine Biology (1994) 118: 767. https://doi.org/10.1007/BF00347527 10.3354/meps268081.
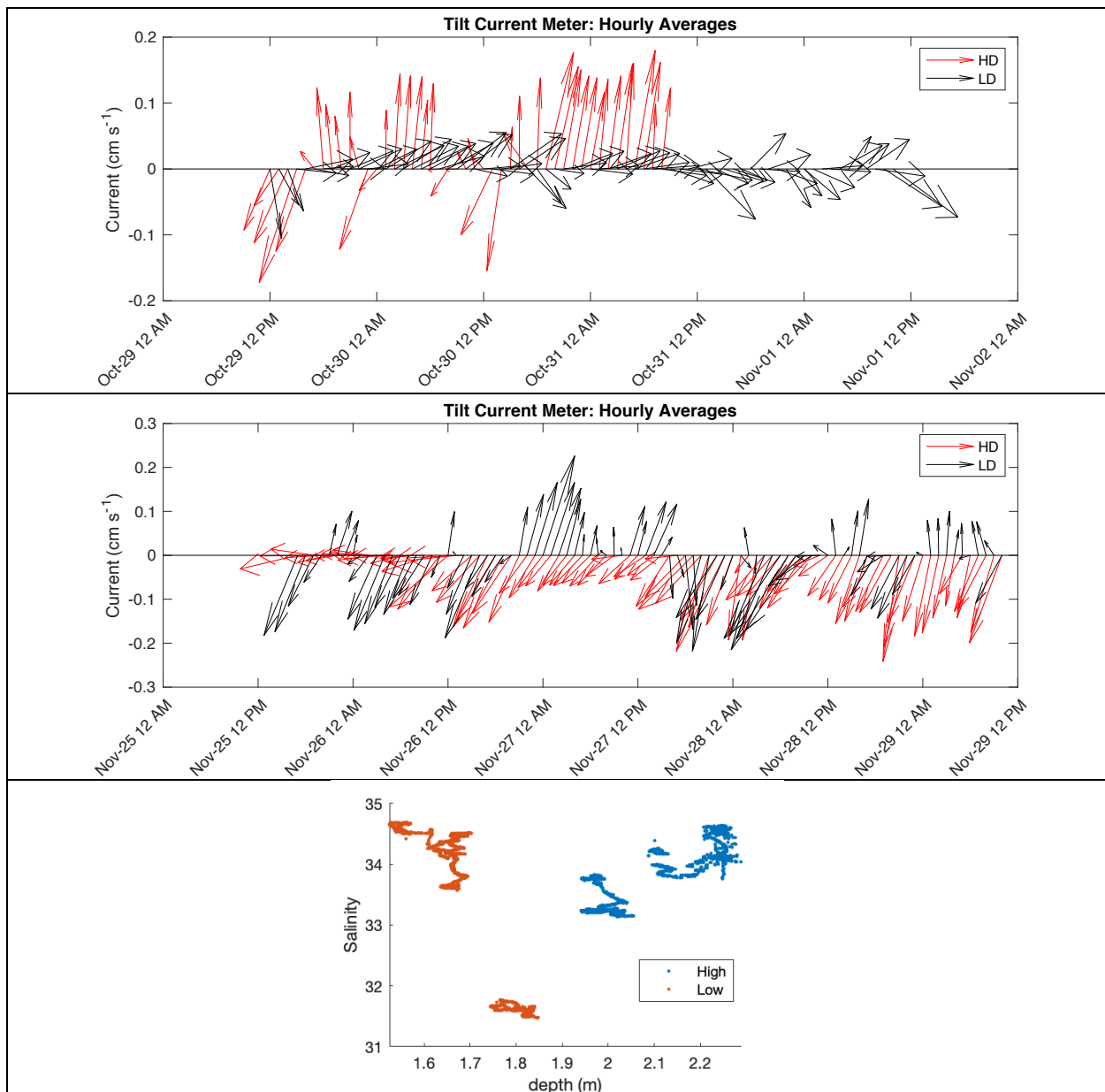
Detailed comments:

Methods:

2.1: Move Table S1 to main text.

Table S1 moved to the main text as table 1

Define "primary sites" here since you reference this phrase. Don't wait until 2.2 to define them.

This term is now introduced in the first sentence of 2.1

2.4: Why such low accuracy on the pH sensors? SeaFETs are capable of accuracy approaching 0.01 pH units or better.

This is the accuracy listed on the manufacturer's website (https://www.seabird.com/seafet-v2-ocean-ph-sensor/product-details?id=54627921732). The precision is indeed much better than 0.05.

2.6: Why the poor precision on the DIC measurements? Please explain.

While TA was analyzed on a commercial instrument, we did not have such a machine for DIC determination. Instead, our DIC measurements were made on a home-made analyzer which consisted of a small impinger filled with 10% HCl, an N2 carrier gas, and a bench-top IRGA

(Licor 6262). There was uncertainty in sample injection, which was done manually, and peak area integration, which was done by the IRGA. While our precision was lower for DIC than for TA, it was still reasonably close to what is achieved by commercial units, which typically achieve ~2 μmol/kg accuracy (e.g. Apollo SciTech ASC3 [http://www.apolloscitech.com/dic.html]). While other instruments like the VINDTA 3C (http://www.marianda.com/index.php?site=products&subsite=vindta3c) claim ~1 μmol/kg precision, reported standard deviations of CRMs are generally higher for both TA and DIC, closer to 2-4 μmol/kg (McMahon et al., 2018; Lemay et al., 2018; Turk et al, 2016, etc…).

McMahon, A., I. R. Santos, K. G. Schulz, T. Cyronak, and D. T. Maher. 2018. Determining coral reef calcification and primary production using automated alkalinity, pH and p CO 2 measurements at high temporal resolution. Estuar. Coast. Shelf Sci. **209**: 80–88. doi:10.1016/j.ecss.2018.04.041

Lemay, J., H. Thomas, S. E. Craig, W. J. Burt, K. Fennel, and B. J. W. Greenan. 2018. Hurricane Arthur and its effect on the short-term variability of p CO 2 on the Scotian Shelf , NW Atlantic. Biogeosciences 2111–2123.

Turk, D., J. M. Bedard, W. J. Burt, and others. 2016. Estuarine , Coastal and Shelf Science Inorganic carbon in a high latitude estuary-fjord system in Canada ' s eastern Arctic. Estuar. Coast. Shelf Sci. 178: 137–147. doi:10.1016/j.ecss.2016.06.006

2.7:Your NEC model does not account for changes in TA due to organic production, despite your acknowledgement in the text and Fig. 3 that TA is influenced by organic matter
 production (see comment below about inconsistencies between delta_TA/delta_DIC ratios for organic production between your text and figure). You need to account for the other processes that influence TA in order to accurately calculate NEC.
Why are you using gas transfer velocity parameterizations designed for open ocean conditions when coastal parameterizations exist? See:
Ho, David T., et al. "Air-water gas exchange and CO2 flux in a mangrove-dominated estuary." Geophysical Research Letters 41.1 (2014): 108-113.
Ho, David T., et al. "Influence of current velocity and wind speed on air-water gas exchange in a mangrove estuary." Geophysical Research Letters 43.8 (2016): 3813-3821.

We chose to apply two separate parameterizations because together they constitute what might be considered a maximum range in k, within which we expect that the actual value lies. Because calculated NEP using these two (excessively) different parameterizations were very similar, we felt justified in reporting a single value from Ho 2006. While we are well aware of the Ho 2016 and Ho 2014 parameterizations, we elected not to use them because of the lack of quality water velocity data, and the fact that currents at our site (likely < 2 cm/s) were at least an order of magnitude lower than the velocities in the tidal river in Ho 2016 (20-40 cm/s). Likewise, Ho et al., 2014 reports average tidal velocities of ~35 cm/s, well outside the range at our site.

Results:

3.1

p. 7, L 17-18: The statement about lateral variations being insignificant because observed changes in SSS of < 1 is only correct if you knew that large spatial gradients in SSS existed and that they were correlated with TA, DIC, etc.

This is a very good point. We have removed 'lateral mixing' from the sentence, and have clarified that we were referring to sources of fresh water, not TA or DIC.

p. 7, L 22-23: Present O2 concentrations, not just percent of saturation (which is temperature and salinity dependent)

DO is now presented as a concentration rather than a percent saturation (Fig 2), and the text references have been corrected as well. The diel trends in DO remain apparent in the figure.

p. 7, L 28-29: t-tests assume independence between data sets, but your CO2 fluxes are likely to be linearly related (since the only difference is the estimated value of the gas piston velocity). I don't think t-tests are relevant since differences in gas flux should simply reflect differences in piston velocity.

We have removed the discussion of CO2 flux t-tests from section 3.

p. 9, L 6-9: When you plot nTA against nDIC, the slope is not nTA:nDIC, but delta_nTA/ delta_nDIC. Please be careful how you describe this in the text.

We have added a brief clarification on this point.

p. 9, L 9-10: When you only have two variables (nTA and nDIC), you can only resolve two processes (production and calcification). Right now, you are trying to resolve four processes (production, calcification, sulfate reduction, and denitrification) with only two variables. Your system is underdetermined.

We very much agree, and have added a sentence at the end of the paragraph reiterating this point.

3.2p. 9, L 21: I do not believe this section is well served by the inclusion of metabolic rate comparisons between this study and previous seagrass metabolism studies. Move the comparisons to the paper Discussion.

Yes, this discussion of metabolic rates in the context of previous studies is not suited for the results section. It has been moved to the discussion section 4.1.

p. 10, L 15-16: This is not the presentation of a statistical test result

These sentences were removed as per Reviewer 1's earlier comments.

Figures

Fig 2: I find this figure very difficult to follow. Multiple data series and and multiple variables along each subplot make it difficult to track what's going on where. Some axes are labeled and some are not. Please consider making additional plots, each with one variable, and labeling all axes. If there are too many resulting plots, you can put some in the supplement.

Fig. 2g,h: Point plots are difficult to track for understanding daily cycles. Recommend connecting points with a line.

We appreciate the advice, and have revised figure 2 to include axis titles for all sub-figures and have connected the points in figures g and h with lines.

Fig. 3: Where do you get the information that TA will decrease as DIC decreases? You reference the classical assumption of slight increases in TA with DIC uptake (p. 7, L 14 and also p. 17, L 16), but you have a positive line in Fig. 3 for TA/DIC relationships for organic production in Fig.

3 and the caption states "...., which generates 0.15 moles of TA for every mole of DIC respired." These two messages are inconsistent. Please clarify.

Reviewer 1 is correct, we should present a slope of -0.15 for the blue line in figure 3 representing TA uptake with productivity on NO_3. This has been corrected in the new figure.

Fig. 4: Same comment as for Fig. 2 about multiple data sets and multiple variables. It is unnecessarily confusing to try to interpret these graphs and impatient readers won't invest much time and energy into attempting to do so. Also, same comment about connecting points with lines as with Fig. 2g,h. Please also provide a figure legend.

We regret that this figure is difficult to follow, but we have tried a number of ways to plot these data and settled on the current display as the least bad representation. On a previous version of this figure, we tried to connect the points with lines, but it became far too busy and difficult to see. We also tried to use box and violin plots, but there simply aren't enough data points to make these plots work.

Fig. 6: Panels d) should be separated (split into a separate figure) from panels a-c) because they show fundamentally different relationships. Panels a-c) show relationships between metabolic rates and PAR. Panel d) shows relationships between oxygen and carbon fluxes during photosynthesis.

We appreciate Reviewer 1's advice, and have split Figure 6 into two separate figures. The in-text references have been revised accordingly.

Fig. 8: Units on x-axis are incorrect. 1/DIC is in units of kg/umol, not umol/kg

Units have been corrected in Figure 8.

Fig. 9: TA:DIC, not DIC:TA (check all labels)

Figure 9 has been removed from the manuscript.

Tables:

The information in Table S1 is key to understanding the differences between the high density and low density sites. At least an abridged version belongs in the main text.

Table S1 has been moved to the main text.

Typos:

p. 2, L 22: Missing "it" between "While" and "is"

p. 7, L 2: Missing a space between "k600" and "parameterizations"

Manzello et al. (2012) reference (not "Manzanello), also correct in-line citation (p. 16, L 23)

We thank Reviewer 1 for catching these mistakes, which we have now corrected.