Summary:

Van Dam et al. present seagrass metabolic rate estimates from two sites within Florida Bay. They found net heterotrophy and evidence for carbonate dissolution with the seagrass meadows and discuss the various drivers and implications of their metabolic rate findings for seagrass buffering of seawater chemistry. There is need for more information about seagrass metabolism and its relationships with water chemistry, so the study is well-motivated. The authors have clearly done a lot of work and I commend them for their effort. However, I have significant concerns about the metabolic rate calculations that constitute the main results of the paper.

I would not be comfortable seeing this paper published until the concerns are sufficiently addressed because I believe that addressing the concerns may change the main results of the paper.

In the first part of the review, I discuss my primary criticism of the study. I provide some detailed comments that pertain to the various sections, figures, and tables in the second part of the review. There is a short list of typos at the end of the review.

Primary constructive criticism:

The metabolic rate estimates are based on the “slack water” approach which considers an isolated pool of water such that changes in water chemistry cannot be attributed to advection or dispersion. Yet the authors do not sufficiently justify their adoption of the slack water simplification. These areas are not tidally isolated (e.g. tide pools), and although they feature low currents (< 2 cm/s; section 2.4), we have no sense of the spatial variation in O2 and DIC that would help us assess how any advective fluxes would compare to fluxes from gas exchange and/or metabolism. In particular, as highlighted by Lowe and Falter (2015), it is difficult to have both a) weak enough currents to minimize advective fluxes and b) strong enough turbulence to sufficiently mix the water column (see reference below).

I want to try to convince you that ignoring small spatial gradients and weak currents could cause you to misinterpret your metabolic rate data by ignoring advective fluxes. As an example, let’s consider a simple advection-reaction model of the TA mass balance at one of the sites (equivalent to Eq. 1 in the paper with a term included for advection):

dTA/dt = u * delta_TA/delta_x - (2 * NEC/ rho * h)

At steady state (dTA/dt = 0), we could simplify this to:

(2 * NEC) / (rho * h * u) * delta_x = delta_TA

Assuming NEC = 5 mmol/m^2/hr (within the range of values presented in Fig. 4), rho = 1025 kg/m^3, u = 1 cm/s, h = 2m, and delta_x = 100m, we can solve for delta_TA and

get:

delta_TA = 13 umol/kg

In other words, just a 13 umol/kg gradient between upstream and downstream TA in a 100m long meadow with a velocity of 1 cm/s (below your instrumental detection limit) would generate an advective flux equivalent to your reported rates of NEC. This TA range is far below your reported ranges in daily TA variability (which may be confounding temporal and spatial variability from advection). I suspect that your metabolic rates are really some combination of metabolism and advection. In some cases ignoring advection may be causing you to underestimate metabolism and in other cases may be causing you to overestimate metabolism.

Without accounting for the role of advection in the TA, DIC, and O2 mass balances within the seagrass meadows, I am not confident in your conclusions about net heterotrophy and net dissolution.

Given that the authors have O2 and pH measurements from some of the other FCE-LTER sites, they should explore how their metabolic rate estimates might change if they considered spatial variation in the biogeochemical parameters and associated advective fluxes (even if currents were < 2cm/s). They could at least put some error bounds on their metabolic rate estimates this way. Such an exercise would be especially doable if you have information on current direction from your tilt meters, even if you don't have current magnitude.

Finally, the authors implicitly acknowledge the role of advection when they discuss TA:DIC export (Fig. 9). The concept of export implies entry and exit flow through a system (in this case, each seagrass meadow), otherwise there would be no export. So how does one rationalize slack water metabolic rates and export at the same time?

Lowe, Ryan J., and James L. Falter. "Oceanic forcing of coral reefs." Annual review of marine science 7 (2015): 43-66.

Detailed Comments
Methods:
2.1: Move Table S1 to main text.
Define "primary sites" here since you reference this phrase. Don't wait until 2.2 to define them.

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

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

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.

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.

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

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.

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.

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.

3.2
p. 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.

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

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.

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.

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.

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.

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

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

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.

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)