Anonymous Referee #3

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General comments

Van Dam et al. present short-term carbonate chemistry variability from two seagrass meadows in Florida Bay. Assessments of net ecosystem productivity (NEP) and net ecosystem calcification (NEC) indicated net heterotrophy and $CaCO_3$ dissolution during eight days in the fall season. Furthermore, the authors compare NEP inferred from dissolved inorganic carbon measurements and oxygen measurements, and discuss reasons for and implications of the observed discrepancy. The study is well-designed and very timely as there is a lack of knowledge on how seagrass systems modify sea- water carbonate chemistry on different temporal and spatial scales. However, although the carbonate chemistry methodology is appropriate, the interpretations and conclusions on TA fluxes and NEC would have benefited from additional measurements of e.g., Ca^{2+} and SO_4^{+}. Without constraining other biogeochemical processes that affect DIC and TA, it should be more clearly indicated that some of the conclusions are associated with uncertainty and are speculative. Provided that the issues raised here are properly addressed, I would be happy to recommend this manuscript for publication. Please see my comments below.

We thank Reviewer 3 for their thoughtful, thorough, and constructive remarks. After considering their comments, we have revised the manuscript in an attempt to more clearly state the extent to which our discussion of NEP and NEC is subject to uncertainty, both with respect to additional sources/sinks of TA, and lateral mixing.

Regarding the reviewer's comment about additional measurements of Ca and SO_4, in fact we did collect samples for Ca^{2+}, which were analyzed on an ion chromatograph. However, due to the ionic strength of these seawater samples, we had to dilute the samples by a factor of well over 100x. Because of the possible error in dilutions of this magnitude, we felt uncomfortable presenting those Ca measurements here. We strongly agree that Ca and SO_4 measurements would have been highly valuable, and regret that we were unable to generate reliable data for the present study.

The Methods section needs improvement. Information is missing on how several variables were measured and what sample sizes were used. Moreover, there is no information on how error propagation was calculated for your flux measurements, which could affect your conclusions. In section 2.1 and 2.2, how do you define your High Density and Low Density sites? Is it based on seagrass shoot density? If so, some quantification of this density would be beneficial for the justification of your site categorization. Above- and belowground biomass and productivity are reported for the two sites in Table S1, but it is unclear if your site categorization is based on any of these variables. Please state this clearly in the Methods section.

The Results section contains speculations and comparisons to previous studies that would be more suitable in the Discussion section. For example, p. 9, line 7-10, line 21; p. 10, line 1-7, line 19-20.

The Discussion section is well-written and easy to follow. However, I am missing some discussion on residence time within your two sites. You state that current flows were low, but no information is provided on tidal regime, prevailing wind direction etc. You briefly state in section 2.4 that current speeds were low (<2 cm s^{-1}), but it is unclear if this means that you treat your sites as closed systems. If not, your budget in Section 4.3 neglects lateral import of DIC and TA from upstream systems as the export flux calculations are based on several assumptions that cannot be resolved with discrete point measurements of only DIC and TA. Aside from this, Section 4.3 brings up very important and relevant considerations for seagrass carbon cycling.

Due to these comments, and those of Reviewer 1, we have elected to remove the budget that was presented in section 4.3. Reviewer 1 also had questions regarding the impact of advection on our metabolism estimates. For a more detailed discussion, please see our response to their comments.

Specific comments

Abstract and Introduction

p. 1, line 10: This is purely semantic but I do not agree that the two seagrass meadows are contrasting. They are the same species, similar physicochemical conditions, similar productivity and water depth (Table S1).

We agree that the main difference between these meadows is indeed limited to biomass and productivity, and have removed the word 'contrasting'.

p. 2, line 28: Seagrass beds and seagrass meadows are used interchangeably. Please use consistent terminology or if you treat these terms differently, please provide an explanation.

'Bed' has been replaced with 'meadow' throughout the manuscript.

Methods

p. 3, line 23-24: Does "aboveground net primary productivity" refer to the data on row three in Table S1? If so, can you really say that they differed with such high and overlapping standard deviations (2.05±0.90 vs. 1.42±1.25)? Were any statistical tests done to test these differences?

In light of the overlapping 95% confidence intervals for productivity (Table 1), we replaced productivity with biomass in this sentence. Primary productivity (as measured by biomass addition) can vary substantially over the short time scales (~1 week) and spatial scales (10s of meters) of studies like this.

p. 4, line 5: Information on how many of the variables presented in Table S1 were measured is missing. For example, how many samples were taken to assess above- and belowground biomass? If only one sample per site was taken, I would be careful to state that they differed in biomass. Similarly, how were sediment carbon and nutrient contents measured. Are the reported C:N:P ratios on mass or molar basis?

Table S1 has been updated to show the number of samples as well as the standard deviation for the analysis used in the main text. Additionally, section 2 now includes the methods as requested. We agree that analyses with only 1 sample are not to be considered for determining site differences, and have included appropriate discussion in the text.

p. 4, line 14-15: This is a bit confusing. Do these dates refer to the measurements of DOC, DIC, and TA for NEPDO, NEPDIC, and NEC or do they refer to air-water gas exchange? If the former, I suggest moving this last sentence up a bit or into the next paragraph where you describe the sampling campaigns.

We apologize for the confusion, and have tried to clarify over what intervals the sampling campaigns lasted.

p. 5, line 5: Is saturation state with respect to aragonite not relevant?

It certainly is relevant, but for simplicity, and because this was not a central point of our manuscript, we chose to present just one carbonate mineral saturation state. Prior studies have shown that the spatial distribution in $\Omega_{calcite}$ and $\Omega_{aragonite}$ look very similar, as does their relationship with salinity (Millero et al., 2001)

Frank J . Millero, William T Hiscock , Fen Huang, Mary Roche, J. Z. Z. 2001. Seasonal Variation of the Carbonate System in Florida Bay. Bull. Mar. Sci. **68**: 101–123.

p. 6, line 1-7: Information on the accuracy of your measurements of DIC and TA is missing. Did you verify your measurements against Certified Reference Material? If you did, please state batch number. The precision of ±5.11 µmol kg-1 is quite poor. Could you provide a possible explanation for this? Were the DIC samples sufficiently preserved (e.g., enough HgCl2)? Also, please add number of samples (n=) for your accuracy and precision assessments.

We have added additional information regarding TA/DIC analysis, including CRM batch number and additional corrections that were made based on CRM measurements. We acknowledge that the +/- 5.11 std dev for DIC is relatively high, but it is still within the upper range for commercial instruments, and we feel that it was sufficient for our purpose. Please see our response to a similar remark from Reviewer 1 for further information regarding TA/DIC analytical uncertainty.

p. 7, line 6-8: What is the unit of k600? cm hr-1? p. 7, line 10: End of sentence is missing.

Yes, we have clarified that we estimated k600 in units of cm/hr.

p. 7, line 17-20: This paragraph is a bit confusing as to what refers to the variation within each deployment and what refers to variation between each field campaign. I would not state that a salinity range from 31.45 to 34.67 is stable, but rather a substantial increase.

We revised this passage for clarification.

p. 7, line 23-24: You have already abbreviated your site names as HD and LD. Please be consistent with site terminology or remove the site abbreviation entirely (HD and LD) as there are already many other abbreviations throughout the manuscript.

We understand that these abbreviations were used inconsistently, and have now removed them from the main text of the manuscript. However, we choose to keep the HD and LD abbreviations in a few of the figures due to space considerations, and to avoid excessive text on the figures.

p. 7, line 23: Please provide DO concentrations instead of just percent.

DO is now presented as a concentration rather than a percent saturation.

p. 9, line 9: These referenced studies did not measure sulfate reduction or denitrification. Please add additional references to back up the statement.

That is very true, our intent was simply to say that prior studies have observed similar relationships between *n*TA and *n*DIC. We have revised the text to hopefully clarify this.

p. 10, line 5: Yes, but see Hines and Lyons 1982 and Holmer and Nielsen 1997.

We thank Reviewer 3 for directing us towards these references, which are now included in section 3.2.

p. 14, line 14-15: Although this is probably correct, I do not think that the observation of high benthic TA fluxes at the bare site necessarily means that sediment redox processes are not important for NEC. Furthermore, although sulfate reduction rates have been found to be higher in seagrass sediments, the oxygen release from seagrass roots can also lead to rapid re-oxidation of sulfide (consuming 1 mol TA).

We agree that this sentence was not well supported, and have removed it.

Hines ME, Lyons WB (1982) Biogeochemistry of nearshore Bermuda sediments. I. Sulfate reduction rates and nutrient generation. Mar Ecol-Prog Ser:87-94

Holmer M, Nielsen SL (1997) Sediment sulfur dynamics related to biomass-density patterns in Zostera marina (eelgrass) beds. Mar Ecol-Prog Ser 146:163-171

Discussion and Conclusion

p. 15, line 2: I suggest you include these productivity numbers in the Results section and also present the high variability (stdev of ±0.9 and ±1.25 μmol m-2 hr-1).

These data are now presented in the results section 3.2

p.15, line 5: Do you consider seagrass belowground productivity as part of the "sediment processes"?

Yes, we certainly do agree that seagrass belowground production is relevant, and have now indicated so in this passage within section 4.1.

p. 16, line 16-18: Were these benthic chambers placed at bare spots within each seagrass meadow or at an adjacent bare site? Porewater chemistry vary on small spatial scales and can be quite different between unvegetated sediments and within the rhizosphere (e.g., due to differences in bioturbation, Corg, O2 release from roots etc.) and if your chamber measurements and δ13C measurements are spatially decoupled I would not combine the two as aggregate evidence.

Chamber measurements were made at bare spots within a few meters of our two main sites. We have updated the methods section 2.5 to make this clear. We understand that soils are highly heterogeneous, but feel strongly that these sediment flux measurements can be considered spatially coupled with our water column chemistry measurements.

p. 16, line 19-21: Yes, but these processes (along with other redox processes) could also affect your NEC estimates. Your TA:DIC ratios are the result of a combination of these processes and without measuring any other reactants and products it is difficult to constrain their contribution to your TA flux. Additionally, organic alkalinity may be produced in the sediments which is not accounted for in TA (see e.g., Lukawska-Matuszewska, 2016).

We agree with the reviewer's point, and have added a sentence to this effect.

p. 16, line 21-24: Yes, indeed. Very well formulated.

p. 17, 2-3: I suggest that these reflections are included in the abstract as well.

We agree that these limitations need to be laid out more clearly in the abstract, and we have now done so.

p. 17, line 10: . . . or throughout the year.

We have now included this remark

p. 18, line 23-24: Very true, but Corg burial operates on much longer timescales than the diel (fall season) NEP and NEC measured in this study.

Agreed; we have revised this sentence to highlight the difference in time scale.

Lukawska-Matuszewska K (2016). Contribution of non-carbonate inorganic and organic alkalinity to total measured alkalinity in pore waters in marine sediments (Gulf of Gdansk, S-E Baltic Sea). Marine Chemistry 186:211-220

Figures

Figure 1 and 2: Please define in the Methods section or figure caption what U10 rep- resents, to help readers who are not familiar with wind speed terminology.

This abbreviation is now listed in section 2.7.

Figure 2: Please place panel letters (a-g) so that they do not interfere with data points.

Panel letters were moved so as to not interfere with data points.

Figure 2g-h: Please use same nTA y-axis range for both campaigns to allow for easier comparison. Following these time series would also be easier if you use lines to connect data points.

These axes were corrected

Figure 3: Why do you not include the slopes for sulfate reduction and denitrification as you mention these processes in p. 9, line 9-10?

We had included lines for sulfate reduction and denitrification in an earlier version of the manuscript, but chose to leave them out here because the figure became too crowded. If the reviewer thinks this would be an important addition, we would be happy to include the extra lines in the future.

Figure 7: This figure is quite confusing to me. The generalized pattern in PPR, [P] and TA is unclear. Does it refer to the sites on the map (e.g., PPR and [P] decreases eastward, TA is high in site BA but low in sites SB, HD and LD?). Please clarify in the figure caption.

We have attempted to clarify the meaning of the generalized pattern at the top of figure 7 (now figure 8). If it is still confusing, we can remove the extra graphics, which are not necessary.

Figure 8: I suggest you move the legend from the inset figure to the main figure and increase the font size. Also, try and increase the size of the dotted confidence interval lines as these are very difficult to see.

Figure 3 has been modified according to reviewer 3's suggestions.

Figure 9: Change "DIC:TA" to "TA:DIC".

This figure was removed

Technical corrections

We thank the reviewer for these technical corrections

p. 2, line 23: Insert "it" after "while"
p. 2, line 30: Change "seagrasses meadows" to "seagrass meadows".

p. 3, line 9-10: Is there a word missing in this sentence? E.g. [. . .], suggesting the "significant/important/negligible" role of NEC or anaerobic catabolic processes in generating excess CO2.

p. 3, line 11-14: Many "potential" in this paragraph. I suggest you remove "potential" from the sentence "discuss potential differences"

p. 5, line 10: Superscript "-1" in mg L-1 and % saturation)
p. 9, line 6: Missing an "and" before "calcification".
p. 10, line 10: Should it not be "[. . .] sampling campaign 1 (a,b) and 2 (c,d)"? p. 16, line 16: Change NEPDIC to NEPDIC.
p. 19, line 2: I do not think coastal Ocean is spelled with a capital O.
p. 19, line 29: Remove "of pH".