

Anonymous Referee #2

The study by Van Dam et al., aims at quantifying net primary production and calcification/dissolution rates of CaCO₃ in Florida bay seagrass meadow. Although the methods used are correct, the study has a major flaw, and from my point of view, the manuscript in it's present form cannot be accepted. The authors are measuring benthic fluxes of TA, in seagrass and sediment, and consider that they are due to calcification or dissolution only (in seagrass, but not in sediment it seems). They therefore ignore all the other redox reactions producing of consuming TA, such as nitrification, denitrification, pyrite burial, sulfite burial, sulfate reduction etc. although those reactions are extremely important in seagrass beds, and indirectly controlled by the seagrass through sediment oxygenation and Corg addition. I strongly advise the authors to read Krumins et al., 2013 (biogeoscience) as well as Sippo et al., 2016 (global biogeochemical cycle). All the part regarding NEC is ill founded. The semi-quantitative arguments proposed by the authors tend to prove that the TA comes from dissolution (TA/DIC ratio and isotopes) are not convincing and only proves that part of the TA only come from this source. Measurements of fluxes of Ca²⁺, by titration, are necessary to quantify NEC. All parts regarding NEC should be removed, and only consider TA fluxes. This is a valuable and much needed data, the article should be rewritten to focus on this. NEC calculations could be proposed in discussion but it will need a very carefull and thorough discussions on sediment processes emitting TA.

Moreover, the study cover only two periods of ~5 days in October and November. This temporal coverage is not sufficient to obtain significant results. More campaigns in other seasons are needed.

We appreciate reviewer 2's constructive criticism, and have made a concerted effort to address their concerns regarding the role of anaerobic processes on NEC. Throughout the manuscript, we have added text reminding the reader when specific results may have been affected by anaerobic TA generation. We have also included extra text throughout that emphasizes the limited temporal scope of the study, and expressed the need for future studies using different approaches over longer time scales in order to confirm or refute our findings. We hope that these changes, along with those that have been made following reviewer 1 and 3's suggestions will be satisfactory for this reviewer.

Some specific comments: Introduction:

P2 : Please develop how calcification emits CO₂.

This sentence was expanded to clarify how calcification generates CO₂.

P2: 4-6: the experiment conducted by enriquez et al., consist in enclosing a piece of seagrass in a very small volume of water exposed to light. This is by no mean a proof that spontaneous CaCO₃ can occur in the field. Besides, from my point of view, the observation of calcification within the tissues of seagrass they did remain to be confirmed.

We agree with the reviewer that more studies are required to confirm that CaCO₃ formation occurs within seagrass tissues and have added phrasing to reiterate this point here.

P2: 34 - 35 : I do not understand that sentence

This sentence was revised to clarify that seagrasses can affect local pH trends by consuming DIC that was generated in adjacent mangroves.

P4: 20. I don't find Karlsson et al., 2017 in the references.

We apologize for the omission; this citation is now included in the reference list.

P5: 11. Did you sampled discrete sample for spectrophotometric pH used for the seafet data validation? See Bresnahan et al., 2014 for example

These SeaFET data were not used to calculate DIC/TA for metabolism assessments, and were simply presented to show the large diel cycles in pH. Our original intent was to estimate NEP/NEC at higher temporal resolution using sensor pH and pCO₂ data, but because we were not confident in the pCO₂ data, we could not do so.

P5: 25. Why using chamber for the bare sediment (and only for the sediment)

The intent here was to isolate the sediment source of TA/DIC by excluding seagrass aboveground biomass, thereby excluding any consumption or production by seagrass aboveground shoots themselves. We have edited this sentence to clarify the point.

P6: 1-10: Did you used "Dickson" CRM
Yes, and this is now explained in greater detail.

P6: 19: Please use the salinity normalization by Friis et al., 2003.

The water budget of Florida Bay is dominated by exchange with the ocean and evaporation and precipitation, which are approximately a factor of 10 greater than surface water inputs which may have a non-zero TA/DIC endmember (Nuttall et al., 2000). Therefore, we believe that the most appropriate approach is to normalize TA and DIC using a zero-salinity endmember, which represents the effect of precipitation and evaporation. Furthermore, the small freshwater input that does enter the northern bay through shark river slough has a highly variable TA concentration, and is located a great distance from our study sites.

P7: 10-14. Please precise the dissociation constants used and evaluate the propagation of error on the CO₂ calculated, using the fct error in seacarb. Please therefore take this error in consideration in subsequent calculations.

We are confident that the largest source of error in our CO₂ flux determination is derived from our parameterization of gas transfer, which is why we used two different equations to estimate k₆₀₀. Furthermore, CO₂ flux represents only a very small fraction (median=1.3%) of the estimated NEP rates. Therefore, we feel confident in presenting the results using a single H₂CO₃ dissociation constant.

P7:11. Why not the latest Schmidt number calculations from Wanninkhof 2014? Please see Sippel et al, 2014.

We regret not using the updated Sc values from Wanninkhof 2014 in our analysis. However, re-doing the entire analysis with the 2014 values would require significant time, and would not appreciably change our CO₂ flux estimates, which are most sensitive to variations in the gas transfer velocity (k₆₀₀), rather than variations in Sc which are small. If reviewer 2 deems it necessary that we re-calculate all metabolism estimates with the updated CO₂ and O₂ Sc values from Wanninkhof 2014, we would of course be willing to do so.

P8: 7. Please express the hours in mean solar time. Fig 4, same.

Time is expressed in local time (EDT or EST) throughout the rest of the manuscript, so we elect to present time in the same format in this figure to avoid confusion.

P15: 9-12. I do not understand this section. The NEP(DIC) you calculate is a production rate of DIC, corrected for air-sea fluxes of CO₂ and calcification (presumably), what is a proper way of doing. It is therefore including the DIC species HCO₃⁻ and CO₃²⁻, how can they escape the calculation?

This section is not intended to say that DIC is somehow 'escaping' the NEP calculation, rather that the large pool of DIC makes NEP calculated with DIC less sensitive to variations in gas transfer than NEP calculated with O₂.

P15: 26. Seagrass themselves? See earlier comment on Enriquez et al., 2014.

Yes. While there is debate over the extent to which seagrass internal calcification occurs, we have mentioned this previously in the manuscript (as per this reviewer's suggestion), and at this point, we also mention other calcifiers which likely contribute in some extent to our NEC estimates.

P15: 16. Your endvalues are far from 0 and close to the range for seagrass Corg. This does not reinforce the argument of TA coming from dissolution.

Indeed, the indicated y-intercept of the Keeling plot does suggest an endmember closer to seagrass Corg. However, the 95% confidence interval for the y-intercept is ~3-11 for the high-density site, and, ~2-16 for the low-density site. This factor, along with the extreme extrapolation involved, means that we cannot confidently say that the endmember is either decidedly "carbonate" or "seagrass OM".

P16: 17. All your measurements are benthic TA fluxes. When it comes from bare sediment, it is a TA flux and when it comes from the seagrass, it is NEC.

We have revised the previous sentence to clarify our intended message that sediment-water TA/DIC fluxes may at times explain a large fraction of measured NEC.

P16: 20. Precisely, and denitrification and sulfate reduction emit TA and is NOT dissolution of CaCO₃.

As per reviewer 2's comments, we have added a sentence expanding on the role of anaerobic processes on TA exchanges.

522. yes, exactly.

All the 4.3 section is dispensable.

As per all 3 reviewers suggestions, section 4.3 was significantly reduced in length and the budget was entirely removed. The remainder of section 4.3 received positive comments from the other reviewers, and we think that it brings up important points, so we elect to keep it in this revision.