

Interactive comment on “Net heterotrophy and carbonate dissolution in two subtropical seagrass meadows” by Bryce R. Van Dam et al.

Anonymous Referee #2

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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 biogeochem-

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ical 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.

Some specific comments: Introduction:

P2 : Please develop how calcification emits 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.

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

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

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

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

P6: 1-10: Did you used "Dickson" CRM

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

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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.

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

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

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?

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

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.

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.

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

522. yes, exactly.

All the 4.3 section is dispensable.

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