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Interactive comment on "Macroalgal metabolism and lateral carbon flows create extended atmospheric CO_2 sinks" *by* Kenta Watanabe et al.

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This is a great study that provides some highly valuable and relevant new insights about the potential transport of macroalgal carbon. Although the export of DOC below the mixed layer is believed to be the main pathway through which macroalgal carbon gets sequestered in the ocean, our understanding of the fate of macroalgal DOC after its release is very limited. This study presents tempting evidence of its potential export to offshore waters (but see some concerns below), which is an important step to verify the role of macroalgae in oceanic carbon sequestration. Overall, I found the study to be well conducted and well written. The authors provide a set of comprehensive measurements of different carbon compartments and forms, which I applaud. Although I am not familiar with some of the more technical protocols of the sample analysis,

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further reading and consulting suggest that they are standard.

One of my principal concerns is that the authors have not yet established a direct transport link between the water exported from the macroalgal bed and the waters at the offshore site. The authors found that (1) water near the macroalgal bed had different properties (namely: lower DIC, fCO2 and higher DOC concentrations) than the water offshore âĂŤ except for February, when DOC concentrations were not significantly different. They then used mass balance models to simulate the diurnal changes in the carbonate and DOC system of the macroalgal bed (ln. 148); incorporating water exchange into their models helped better explain their readings (ln. 218, 245), which suggests that (2) there is water inflowing and outflowing at both the macroalgal bed and offshore site. There is however no direct demonstration that it is specifically the macroalgal bed water the one that reaches the offshore waters. This is a very important nuance, as the water that lowers the CO2 concentrations and enhances atmospheric CO2 uptake at the offshore site could come from other habitats that "produce" low DIC, high DOC water (e.g. seagrass meadow). Characterizing the DOC profile of both waters could help shed light on the provenance of that water.

The mass balance models only consider changes due to processes related to macroalgal metabolism, but some could argue that they are missing some parameters. For example, volatile and semi-volatile compounds can be an important fraction of the DOC, and can be volatilize to the atmosphere (Ruiz-Halpern, Vaquer-Sunyer, & Duarte, 2014) instead of remaining in the water column as assumed here. Similarly, some of the other processes that can affect the DIC pools (e.g. dissolution, chemical addition; (Langdon et al., 2003)), are not considered. If the authors consider that those fluxes are negligible that is fine, but they should provide evidence to back their approach.

It is very valuable that the study measurements were conducted at two separate time points âĂŤalbeit in the same seasonâĂŤwhich gives an idea of the variability associated with the carbon flows estimated in the study. For instance, both the amount of DOCM and its constituents (as suggested by the different decomposition rates) were different

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across months (Wada et al., 2008). These points should be further elaborated to produce a rich and interesting discussion section. It would also be worth discussing how other species of macroalgae may differ in the production and characteristics of their DOC, as S. horneri was not the dominant species in the bed. Another limitation worth discussing is that DOC incubations for the degradation experiments were also maintained at a constant temperature (22), which may not necessarily reflect conditions in the field.

Finally, some of the sections of the manuscript also need to be further clarified, as it is difficult for the reader to grasp how some very key parameters where calculated. For example, it is unclear how the gross community production, respiration and calcification were calculated from the DOC bag experiments (ln. 160), all of which are key parameters in the model. It is also not very clear how the tidal water exchange (EXtide) rate was estimated from changes in depth (ln. 169)

Specific comments

Ln 33: Add “far” before “been”

Ln. 37 Add “more” before “efficiently”

Ln 45: stored where? In the sediments, water column...? Also, consider citing here (Queirós et al., 2019), which provides an example of macroalgal-sediment connectivity.

Ln. 52: I suggest making the topic sentence of the paragraph the fact that DOC is believed (at least according to (Krause-Jensen & Duarte, 2016)) to be the principal pathway of macroalgal carbon sequestration (although). This will highlight more the relevance of this study, as more empirical support is needed to demonstrate the assumptions of (Krause-Jensen & Duarte, 2016)

Ln. 55: This paragraph feels a bit out of place here, you are talking about DOC and all of a sudden start talking about the carbonate system. Consider rearranging/rewriting.

Ln. 61: The sentence gives the impression that the effects of macroalgal metabolism in their surrounding waters have not been studied, which is not the case (the authors provide plenty of examples). What is truly novel is examining its effects on other water bodies. I suggest deleting “both macroalgal beds and”

Ln 67: Sargassaceous algae sounds a bit strange to me, perhaps just use Sargassum beds? Sargassums are also commonly found in tropical regions (Fulton et al., 2019), so I would suggest changing for “we focused on Sargassum beds because they are one of the dominant macroalgal habitats in temperate and tropical regions).

Ln. 69 The issue of carbon sequestration was not directly addressed in this paper, as no evidence that the carbon measured is locked away from the atmosphere for very long periods of time (decades-centuries) is presented. Although some of the DOC did not decompose after 150 days under constant experimental conditions, it is not known how long it would remain in the field or whether it could reach the mixed layer. I suggest cutting similar claims made throughout the ms

Ln. 75. Given that the water inflowing and outflowing from the bed is so important for this study, the readers would appreciate more details about the water movements around the study area (e.g. tidal characteristics, exposure)

Ln. 79. This sentence is a bit redundant from the one in Ln 76. Consider merging them.

Ln. 96. Is that the volume of seawater in the bag?

Ln. 109. Please indicate the pore size of the filter. Was the filtering pressurized?

Ln. 127. What concentration of KHPh?

Ln 140. At what height was the wind speed measured at Agenosho?

Ln 143. Delete “that”

Ln. 148. Using the active voice is more readable in this instance. “We simulated the diurnal changes and budgets of the carbonate system and DOC in the macroalgal bed using mass balance models”

Ln. 151. This sentence seems to indicate that you changed the depth of the macroalgal bed. Please rewrite. Was the tide simulated by changing water height over the bed?

Ln 152. The average Sargassum biomass used was derived from the field surveys, right? Please state so

Ln 157. The amount of formulas, acronyms and parameters used in the manuscript can be a bit overwhelming. I encourage the authors to consider making a first figure with a schematic diagram of the different carbon pools and fluxes, as well as different carbon forms (e.g. POC, PIC, DOC, DIC. . .) and the processes that affect them (e.g. primary production, calcification. . .). That figure could include the formulas in lines 157-159 to show how they were calculated in the mass balance models. I think this could be very useful to the reader.

Ln. 160. It is very unclear how all these parameters where calculated. Did you use some sort of relationship between DOC release and productivity? Please provide further details.

Lns 165-166. They can be just one sentence

Lns 192-193. They can be just one sentence

Ln 205. The use of “g WW” is more standard. Also wet weight (WW) needs to be abbreviated somewhere in the paper.

Ln. 208-209. Please provide statistical evidence that the decrease in time is statistically significant.

Ln 210. Perhaps it would be informative to include those final percentages in Fig. 4, as the decrease is a bit hard to observe in some panels (e.g. 4b)

Ln. 218. Please provide an index of how well the model fits the data. This way you can say that a model improves or worsens by adding/removing water exchange.

Ln 238. Add “For example”, before DIC uptake”

Ln. 168: The estimation of water exchange is crucial for the aims of this paper. I am having a bit of trouble understanding how you EXtide was estimated from changes in depth. Is that referring to tidal height? It could be helpful if some example values are provided (e.g. is the number greater on spring tides, what is the maximum value it can attain? 1? What would that mean)

Ln. 256. I wonder how seasonality will affect the fate of the DOC released as well. How do oceanographic conditions vary in the study area?

Ln 274. You may also be interested in the extensive work of Sophie Martin in maerl beds e.g. (Martin, Clavier, Chauvaud, & Thouzeau, 2007)

Ln. 296-297. These two statements seem contradictory

Ln. 306. Very interesting find!

Ln. 320. Insert “considered as” before “are”

Ln. 321. Consider “[. . .] export of particulate macroalgal carbon (e.g. entire thalli and fragments) to the deep sea [. . .]”

Figure 4. Consider stating the percentage of DOC remaining in each of the treatments of panels 4a and 4b as it is a big hard to tell how much remained sometimes. Also consider shading the area between the two treatments and indicating that it corresponds to the macroalgal DOC (DOCM; ln. 121).

Figure 5. I think that plotting the value of EX in this graphs would be very valuable, as it would help the reader understand what is the water doing (inflow or outflow), and how this affects the readings at the macroalgal bed and offshore sites. The mass balance model should also predict the observations at the offshore site; please plot those ones as well.

Figure 6. I suggest putting a dashed line through the middle of the panes to

clearly delineate the offshore waters from the macroalgal bed. Also, put the titles of “Offshore” and “Macroalgal bed” at the very top so it is easier to read. I think that using symbols instead of the photo of the macroalgal bed would declutter the figure and make it more understandable. For instance, the ones at https://ian.umces.edu/imagelibrary/displayimage-search-0-4529.html are freely available (with attribution) and make for very appealing figures.

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