Review for "Blue Carbon Stocks and Exchanges Along the Pacific West Coast" by M.A. Ward et al. submitted to Biogeosciences

This paper reported organic carbon (OC) stock and accumulation rate in sediments of seagrass beds and salt marshes in less-represented region of Pacific Coast of California, and also evaluated lateral exchange of OC from seagrass beds to adjacent salt marshes in Tomales Bay. The authors showed that salt marshes had higher OC stocks and sequestration rates than adjacent seagrass meadows, and that seagrass wrack transported from the latter to the former did not contribute significantly to OC stock in the former. They also succeeded in providing convincing hypotheses to explain their findings. The overall study is well arranged, and analytical and statistical methods seem to be sound and appropriate. I appreciate that outputs of this study are highly valuable and deserve publication in Biogeosciences. My specific comments are below. I hope the authors can respond to them to improve the manuscript.

1. In the mixing model for source evaluation, the authors used as indicators $\delta^{13}C$ and N/C ratio avoiding $\delta^{15}N$ because $\delta^{15}N$ may be altered during diagenesis (L.238-241). However, to my experience, N/C ratio can shift during diagenesis as well as $\delta^{15}N$. The mechanism of the shift is almost same for both $\delta^{15}N$ and N/C ratio, i.e., selective remineralization of N over C (associated with significant isotope fractionation) and uptake of external DIN and N_2 (with different isotopic signatures) for bacterial growth during diagenesis. So, I think there is no strong reason to choose N/C ratio over $\delta^{15}N$ in the mixing model. Furthermore, they used only two indicators ($\delta^{13}C$ and N/C) for evaluating strengths of four different OC sources (seagrass, C_3 marsh plants, C_4 marsh plants, planktonic+benthic microalgae). In such a case, source strengths cannot be determined analytically, but only guessed as most probable attribution by some stochastic models like SIAR. Therefore, their conclusion that most OC stored in sediment was derived from C_3 plants and microalgae is not a decisive one, but a most probable possibility that was expected by a model containing a perhaps inappropriate indicator such as N/C. This fact somewhat discounts convincingness of the conclusion.

2. Although the authors agreed that sediment grain size is a key driver in OC storage as suggested repeatedly by preceding studies, they also suggested that it may be of limited use as a predictor when mud content exceeds 36% citing Fig. 4 (L.469-471). To my impression, however, Fig. 4 shows that sediment organic matter content (% TOM) clearly correlates with

mud content (% Mud) up to about 80% Mud, with a few outliers. The correlation suddenly gets worth when % Mud exceeds 85%. To my experience (using density-fractionation techniques; cf. https://doi.org/10.1002/lno.10478), the correlation is pretty good as far as most of sediment OC is present as mineral-associated OC (like most seagrass bed sediments), while it gets worth and % TOM increases much in excess as the fraction of OC present as independent organic particles increases (like mangrove soils). I think the results shown in Fig. 4 can be explained similarly.

3. By comparison with previous studies, the authors suggested that California seagrass and marsh sediments they studied have higher OC storage than regional estimates encompassing the U.S. west coast (L.480-488; 499-501). They compared OC stock in the top 1 m of sediment, and it seems that they calculated the OC stock in their sites by simply extrapolating the mean volumetric OC concentration of top 20 cm to 1 m. I think this method may lead to a significant overestimation of the OC stock, because roots and rhizomes of seagrasses and marsh plants are concentrated in top 20 - 50 cm of sediment and the OC concentration in this top layer can be significantly higher than the layer below 50 cm due to accumulation of organic exudates and dead root fragments. Therefore, to accurately compare their results with preceding studies, they should pay attention to possible difference in the extrapolation method for estimating the top 1 m OC stock.

Minor questions and suggestions:
L.201: VPDV –> VPDB
L.215: Section 2.2 is duplicated (see L.184).
L.371: Strictly speaking, wrack is not "biomass" but dead organic matter (necromass).
L.405-406: Colors seem to me somewhat different.
L.431-433: How was the carbon accumulation rates measured in O'Donnell et al. (2017)?
L.495: not surprising (?)
L.546: "biomass" is duplicated.
L.553: Here do you mean emission of carbon dioxide?
Table S1: Unit of OC, k/gm^3 –> kg/m^3
The following references cited in the text do not appear in the References list: McLeod et al. 2011 (L.40, 129); Green and Short 2003 (L.57); Röhr et al 2018 (L.60 and Table 3); Prentice et al. 2020 (L.68 and Table 3); Johannessen and Macdonald 2016 (L.71); Macreadie et al. 2018

(L.72); Alongi et al. 2018 (L.130); O'Donnell et al. 2017 (L.150 and many other); Schlosser and Eicher 2012 (Table 1); County of Orange 2019 (Table 1); Howard 2014 (L.187); Dean 1974 (L.197); Fourqurean et al. 1997 (L.224); Miyajima et al. 2017 (L.457); Jepson Flora Project 2020 (L.540).

The following papers listed in References seem not to be cited in the text: Johnson et al. 2015 (L.739); Serrano et al. 2012 (L.857).