Reviewer #1

Remark 1: the "pacific west coast" ranges from Alaska to Patagonia, so I suggest adding in the title "California coast" or "southwest US coast" or something similar.

Response: We agree, and changed the title to more explicitly address the region under study ("Blue Carbon Stocks and Exchanges Along the California Coast")

Remark 2: I suggest including the definition of Blue Carbon as in Lovelock and Duarte 2019, Biology Letters (https://royalsocietypublishing.org/doi/10.1098/rsbl.2018.0781).

Response 2: The reference provided for the definition of Blue Carbon was added. See line 39.

Reference: Lovelock, C. E. and Duarte, C. M.: Dimensions of Blue Carbon and emerging perspectives, Biology Letters, 15, 20180781, https://doi.org/10.1098/rsbl.2018.0781, 2019.

Remark 3: There needs to be an explanation on whether the authors think that all the SOC stock was accounted in their calculations E.g., clarify in Table 3 at what depth the SOC stocks were estimated for every study.

Response 3: To address this comment, we added text and references for other studies that apply similar methodologies and depth selections to those we use. We cite that OC content has been shown to remain relatively constant below 10 cm to depths up to 1 m (Callaway et al., 2012; Prentice et al., 2020; Fig. S3), providing further justification for a 20 cm core depth selection with an extrapolation up to 1m for comparison across studies. These text additions can be found in lines 206 to 212.

To further address this comment, we add in a description of the core depths over which each value in Table 3 was extrapolated from. In some of the studies referenced in Table 3 (Rohr et al. 2018; Prentice et al. 2020, and to a certain extent Fourqurean et al. 2012), the core collection depths were similar to those in the presented study, making for easy comparisons with our data. However, in other cases, where full meter core inventories were available, the addition of the last column in Table 3 can clarify differences in methodologies when making comparisons across studies.

References: Callaway, J. C., Borgnis, E. L., Turner, R. E. and Milan, C. S.: Carbon sequestration and sediment accretion in San Francisco Bay tidal wetlands, Estuaries and Coasts, 35(5), 1163–1181, doi:10.1007/s12237-012-9508-9, 2012.

Fourqurean, J. W., Duarte, C. M., Kennedy, H., Marbà, N., Holmer, M., Mateo, M. A., Apostolaki, E. T., Kendrick, G. A., Krause-Jensen, D., McGlathery, K. J. and Serrano, O.: Seagrass ecosystems as a globally significant carbon stock, Nature Geoscience, 5(7), 505–509, doi:10.1038/ngeo1477, 2012.

Prentice, C., Hessing-Lewis, M., Sanders-Smith, R. and Salomon, A. K.: Reduced water motion enhances organic carbon stocks in temperate eelgrass meadows, Limnol Oceanogr, 64(6), 2389–2404, doi:10.1002/lno.11191, 2019.

Röhr, M. E., Holmer, M., Baum, J. K., Björk, M., Boyer, K., Chin, D., Chalifour, L., Cimon, S., Cusson, M., Dahl, M., Deyanova, D., Duffy, J. E., Eklöf, J. S., Geyer, J. K., Griffin, J. N., Gullström, M., Hereu, C. M., Hori, M., Hovel, K. A., Hughes, A. R., Jorgensen, P., Kiriakopolos, S., Moksnes, P.-O., Nakaoka, M., O'Connor, M. I., Peterson, B., Reiss, K., Reynolds, P. L., Rossi, F., Ruesink, J., Santos, R., Stachowicz, J. J., Tomas, F., Lee, K.-S., Unsworth, R. K. F., and Boström, C.: Blue carbon storage capacity of temperate Eelgrass (*Zostera marina*) meadows, Global Biogeochemical Cycles, 32, 1457–1475, https://doi.org/10.1029/2018GB005941, 2018.

Remark 4: I wonder whether grain size affects SOC or whether is the other way around. Would higher root production filling in the "accommodation space" (as in Rogers et al. 2019, Nature, https://www.nature.com/articles/s41586-019-0951-7) would result in less available space for mineral accumulation? This may also result in differences in grain size with SOC stocks.

Response 4: We agree with this suggestion and additional explanations for variation in grain size and SOC were added in text. See lines 464 to 466.

Reference: Rogers, K., Kelleway, J. J., Saintilan, N., Megonigal, J. P., Adams, J. B., Holmquist, J. R., Lu, M., Schile-Beers, L., Zawadzki, A., Mazumder, D., and Woodroffe, C. D.: Wetland carbon storage controlled by millennial-scale variation in relative sea-level rise, 567, 91–95, https://doi.org/10.1038/s41586-019-0951-7, 2019.

Remark 5: The interpretation of changes in SOC with depth as degradation rate is interesting, but I am not sure if it is correct. For example, the differences in depth within one of the saltmarsh in Newport Bay could just be because the top sediment is mostly fine (live or dead) roots and may have nothing to do with high degradation. Contrary, vertically homogenous cores at all seagrass sites may just mean that there is not many roots in the top sediment, not that degradation is low. I think that changes in SOC with depth could be a good indicator of degradation, but only once live roots have been accounted for, and also with deeper cores.

Response 5: We agree and have removed language definitively attributing the changes in SOC to degradation. Specifically, we additionally suggest that changes in downcore trends could be due to minimal changes in factors such as vegetation or grain size over time (lines 429-430), or similarly due to changes in local hydrography that alter SOC (lines 431-432).

Remark 6: While the results show that most of the carbon within the marsh was a combination of diatoms AND marsh, I would argue that its actually diatoms AND/OR marsh. Because the mixing models will include in the results as many input sources, and because these two sources (Diatoms and marsh) are overlapping, there is no way to know whether is one and the other or just one source. I would think that due to fact that diatoms are extremely refractory and a very nutritious food, they would be rapidly consumed either in the water column or when deposited in the sediment (e.g., snails). I would just suggest leaving open various possibilities to the interpretation of the isotope model.

Response 6: We agreed with the comments provided here, which were also echoed by Reviewer 2. To allow for the fact that C3 marsh plants and diatoms are similar sources, we instead pool these two sources and apply a 3-source, 2-tracer model (see lines 265-270). This model reflects that sediment contributions may be diatoms "AND/OR" C3 plants in the salt marsh sediments studied here. After adjusting the model according to this recommendation, we have updated all figures, statistics, and text. While this change required that we soften some conclusions (see lines 399-402, and 526), it does not alter the key conclusion that "the lack of seagrass-derived carbon in underlying sediment makes a compelling case that little of this material is ultimately buried".

Reviewer #2

Remark 1: In the mixing model for source evaluation, the authors used as indicators δ13C and N/C ratio avoiding δ15N because δ15N may be altered during diagenesis (L.238-241). However, to my experience, N/C ratio can shift during diagenesis as well as δ15N. The mechanism of the shift is almost same for both δ15N and N/C ratio, i.e., selective remineralization of N over C (associated with significant isotope fractionation) and uptake of external DIN and N2 (with different isotopic signatures) for bacterial growth during diagenesis. So, I think there is no strong reason to choose N/C ratio over δ15N in the mixing model.

Response 1: To the authors knowledge, both C/N ratios and δ^{15}N are suitable for use in mixing models. Some research suggests that while diagenesis can modify the C/N of organic content in sediment, it may not severely alter the ratio, although we acknowledge this evidence may not be definitive. For example, Craven et al. (2017) writes "*In saltmarshes, while d13C is considered relatively conservative and not susceptible to large diagenetic fractionation in sediment, d15N is rapidly altered during early diagenesis (e.g. Benner et al. 1991) and is not an appropriate SIAR system tracer unless diagenetic fractionation is quantified. In its place, the absolute ratio of C/N can be used (e.g. Goñi et al. 2003; Gordon & Goñi 2003; Liu & Kao 2007), as this is more resistant to diagenetic changes (Lamb et al. 2006).*" For this reason, we choose to use C/N ratios as a tracer in our model, although it is likely that either C/N or δ^{15}N would be feasible.

References: Craven, K. F., Edwards, R. J. and Flood, R. P.: Source organic matter analysis of saltmarsh sediments using SIAR and its application in relative sea-level studies in regions of C 4 plant invasion, Boreas, 46(4), 642–654, doi:10.1111/bor.12245, 2017.

Remark 2: They used only two indicators (δ13C and N/C) for evaluating strengths of four different OC sources (seagrass, C3 marsh plants, C4 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 C3 plants and microalgae is not a decisive one, but a most probable possibility

Response 2: We agreed with the comments provided here, which are also in response 6 above. Specifically, to allow for the fact that C3 marsh plants and diatoms are similar sources, we instead pool these two sources and apply a 3-source, 2-tracer model (see lines 265-270). This model reflects that sediment contributions may be diatoms "AND/OR" C3 plants in the salt marsh sediments studied here. After adjusting the model according to this recommendation, we have updated all figures, statistics, and text. While this change required that we soften some conclusions (see lines 399-402, and 526), it does not alter the key conclusion that “the lack of seagrass-derived carbon in underlying sediment makes a compelling case that little of this material is ultimately buried”.

Remark 3: 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.

Response 3: The 36% value cited in the manuscript is a product of the way analysis was done, showing statistical significance of the trendline when all possible values of TOM are included (Figure 4). Based on the comments and references provided by the reviewer, we altered our analysis, and produce a more realistic estimate of the threshold at which the correlation between % mud and % total organic material is no longer significant. Specifically, we selectively filter out sediments containing higher % mud, finding a single abrupt change in r^2 value as soon as sediments consisting of more than 82% mud are included in analyses. This threshold value (82% mud) is very close to what is suggested by reviewer 2. See lines 340 to 342.

To further address this, we also add a reference in text citing that in salt marsh habitats, the root systems of overlying vegetation add bulk organic material into available sediment space, contributing to increased carbon deposition as well as decreased space for mineral accumulation (see lines 464-466).

Reference: Rogers, K., Kelleway, J. J., Saintilan, N., Megonigal, J. P., Adams, J. B., Holmquist, J. R., Lu, M., Schile-Beers, L., Zawadzki, A., Mazumder, D., and Woodroffe, C. D.: Wetland carbon storage controlled by millennial-scale variation in relative sea-level rise, 567, 91–95, https://doi.org/10.1038/s41586-019-0951-7, 2019.

Remark 4: 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.

Response 4: This same general comment was echoed by Reviewer 1 and addressed in Response 3 above.

To address this comment, we added text and references for other studies that apply similar methodologies and depth selections to those we use. We cite that OC content has been shown to remain relatively constant below 10 cm to depths up to 1 m (Callaway et al., 2012; Prentice et al., 2020; Fig. S3), providing further justification for a 20 cm core depth selection with an extrapolation up to 1m for comparison across studies. These text additions can be found in lines 206 to 212.

To further address this comment, we add in a description of the core depths over which each value in Table 3 was extrapolated from. In some of the studies referenced in Table 3 (Rohr et al. 2018; Prentice et al. 2020, and to a certain extent Fourqurean et al. 2012), the core collection depths were similar to those in the presented study, making for easy comparisons with our data. However, in other cases, where full meter core inventories were available, the addition of the last column in Table 3 can clarify differences in methodologies when making comparisons across studies.

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Remark 5: L.201 VPDV –> VPDB

Response 5: Done as requested (line XYZ)

Remark 6: L.215 Section 2.2 is duplicated (see L.184).

Response 6: Done as requested (line XYZ)

Remark 7: L.371 Strictly speaking, wrack is not "biomass" but dead organic matter (necromass)

Response 7: We choose to continue our use of 'biomass' rather than 'necromass' (line 371). We feel that when defined as wrack (line 230), it is understood that material is not live and reduces the use of jargon in text. We find use of similar language in previously published work (e.g., Wasson et al. 2017; Lavery et al. 2013).

References: Wasson, K., Jeppesen, R., Endris, C., Perry, D.C., Woolfolk, A., Beheshti, K., Rodrigues, M., Eby, R., Watson, E., Rahman, F., Haskins, J., and Hughes, B. Eutrophication decreases salt marsh resilience through proliferation of algal mats. Biological Conservation, 212, 1-11, http://dx.doi.org/10.1016/j.biocon.2017.05.019, 2017.

Lavery, P., McMahon, K., Weyers, J., Boyce, M., and Oldham, C.: Release of dissolved organic carbon from seagrass wrack and its implications for trophic connectivity, Mar. Ecol. Prog. Ser., 494, 121–133, https://doi.org/10.3354/meps10554, 2013.

Remark 8: L.405-406: Colors seem to me somewhat different.

Response 8: Done as requested, colors for Figure 6 and 7 have been updated to match more perfectly.

Remark 9: L.431-433 How was the carbon accumulation rates measured in O'Donnell et al. (2017)?

Response 9: We add additional methods detail in parentheticals in Line 439

Remark 10: L.495 not surprising (?)

Response 10: Done as requested (line XYZ)

Remark 11: L.546 "biomass" is duplicated.

Response 11: Done as requested (line XYZ)

Remark 12: L.553 Here do you mean emission of carbon dioxide?

Response 12: We clarify that in line 553, the text is correct as written, we mean emission of carbon.

Remark 13: L.553 Table S1: Unit of OC, k/gm^3 –> kg/m^3

Response 13: Done as requested (line XYZ)

Remark 14: 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).’

Response 14: Done as requested, all references have been added

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Remark 15: 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).

Response 15: Done as requested, all references have been removed