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Comment on bg-2021-27

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Referee comment on "Blue carbon stocks and exchanges along the California coast" by Melissa A. Ward et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-27-RC1, 2021

The manuscript by Ward et al presents a nicely written, coherent study on carbon stocks and fluxes across the temperate California coast in the USA. While the data on carbon stocks is not particularly novel, the additional analyses of the data with isotope tracers and the comparison among vegetation types provide interesting insights into the carbon fluxes within this coast. I have some comments and suggestions that can hopefully improve this manuscript.

Title: 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.

Introduction: This section is nicely written. 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).

Methods: Because the cores were quite shallow (20 cm), there needs to be an explanation on whether the authors think that all the SOC stock was accounted in their calculations. In my experience (although mostly in tropical locations), SOC is usually deeper than 20 cm. It is relatively easy to identify in the field when most of the organic matter is accumulated with a one-meter sediment corer. If the total depth of SOC is unknown, the comparison among other sites should be reconsidered to only account for carbon up to 20 cm deep, e.g. clarify in Table 3 at what depth the SOC stocks were estimated for every study.

Results: The correlation between SOC and grain size has been published before, but it is still interesting. 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.

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.

I thought it was really interesting the section on the effects of seagrass wreck on saltmarsh SOC, I have seen these wreck in saltmarsh in before in Baja California and was intrigued by the same question. The data collected from the authors provided a good explanation and evidence for little accumulation of seagrass in marshes.

My last comment is on the isotope mixing model interpretation. 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.

Nice and clear conclusion

Good luck!

Fernanda