Fettrow et al. assessed C concentrations in relation to different important biogeochemical parameters at three hydrodynamically distinct sites in a coastal marsh system over the course of the course of four vegetation periods. They find that total C and DOC concentrations as well as DOC across the sites vary with biogeochemical regime across the hydrological gradient and with vegetation period. They argue that the variability in C concentrations across the sites and with depth should be taken into account when blue carbon assessments are considered.

Main comment:

The authors provide a really nice biogeochemical dataset that builds on prior in-depth characterization of the same sites ((Seyfferth et al. 2020; Guimond et al. 2020). Particularly the comparison of tidal influence versus vegetation dynamics at the three different sites yields some interesting results. I think those results are novel and useful, and should be the core of the manuscript. While I agree that C stock assessments in these kinds of systems are tricky, the study was not set up to rigorously evaluate spatial variability across these kinds of systems. Three distinct sites seem to be deliberately chosen based on their distinct hydrological regimes (see prior work cited above) and three replicate cores at each site are just not enough to truly assess spatial heterogeneity across the sites. My suggestion is to focus the revised and streamlined version of the manuscript on how tidal versus vegetation dynamics might affect total C/DOC concentration and DOC composition. There are some interesting results there that should make for a much tighter and interesting paper. In other words, focus on the spatiotemporal variability of C/DOC from a biogeochemical perspective. The fact that it makes for difficult C stock assessment is an interesting discussion point or implication, but perhaps shouldn't be the focus of this research article.

Other general comments:

- I noted below that ANOVA results are missing from Fig. 1-4. But then they were presented in Table 1-2. I suggest really shortening the results sections around Figs. 1-4 and incorporating the ANOVA results there. It is otherwise redundant, and the information provided doesn't always seem directly relevant for the questions asked and hypotheses posed.
- The authors discuss C storage rates, but what you are measuring is C concentrations and what you are estimating seems to be stocks. The term carbon storage invokes that whatever carbon is there is persistent and stored. I would only use it where appropriate. Generally, the whole text would benefit from more clearly delineating when C accrual, storage, concentrations or stocks are discussed. Or when the authors talk about pools (stocks, concentrations,…) and rates.

ABSTRACT

Put more emphasis on results and less on hypotheses and approach.

L41: maybe “plant phenology” instead of “ecological function”?

Maybe end on recommendation for sampling if one is interested in estimating/assessing C stocks?

INTRO

L86: exudates are by definition soluble, so perhaps omit “DOC”

Perhaps try to more clearly delineate the edaphic versus the plant controls on soil C stocks.

METHODS

157f: I assume the cores are extremely wet and take a long time to dry, especially given the high organic matter content. Wouldn’t there by anaerobic metabolism in the glove bag, it’s warm and wet in there, particularly in the presence of H2? So couldn’t stem some of the seasonal variability in C content stem from differences in microbial activity at the time of sampling that then dictates how much C metabolism occurs in the glove bag? It seems like freeze-drying might be a better alternative.

180f: Is this a water extraction or really an extraction of the residual pore water in the cores? If it is the former, perhaps call it water extractable C. If it is the latter, isn’t the extracted DOC concentration highly dependent on the moisture content at the time of sampling? And that moisture content will be a function of where in the tidal cycles it was sampled? Is it possible that the variability has more to do with that than site or season specific characteristics.

RESULTS

I don’t quite understand why Fig. 2 and Fig 3 are necessary. I think the variability is nicely illustrated by Fig. 3. I would also add symbols indicating significant (where appropriate) in the latter.

212-214: if such a statement is made, it should be supported with adequate statistics

225-229: same as above

229-232: The regression approach is a very forgiving way to assess significant changes with depth. I think it would be more appropriate to run a ANOVA. But, frankly, I don’t really see how they are significantly different given the large variation among the three reps.

239-243: Again, it's ok to point out trends, but if it is claimed they are different, there should be statistical tests to support that claim.

Fig. 4: I would suggest plotting DOC concentrations analogous to Fig. 3, i.e., as a box plot and run the appropriate statistics. This data is really neat and I would like to see it highlighted like that.

276-277: why isn't this discussed? Wouldn't it make sense to highlight differences across the sites as well?

Fig. 6: I don't love this figure. Could you make the lines a bit thinner so it's easier to see the individual traces? Everything is also very compressed. For example, Eh varies quite a bit with season, but it's hard to see because the scale is so compressed.

Table 1-3 header: Soil C % is not really a porewater biogeochemical variable. The table includes the solid phase.

Also, wouldn't a two way ANOVA be more appropriate to assess the influence of both vegetation and season?

378f: it would help to better explain the step-wise linear regression approach. Which factors were included and which were eliminated in the process?