Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2019-106-RC1, 2020

Ocean Science
Discussions
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Interactive comment on "Spatiotemporal variability of light attenuation and net ecosystem metabolism in a back-barrier estuary" *by* Neil K. Ganju et al.

Anonymous Referee #1

Received and published: 10 February 2020

The manuscript by Ganju et al. uses long-term high frequency measurements to quantify biogeochemical dynamics, coherence and metabolism in a shallow back-barrier estuary with focus on the role of submerged aquatic vegetation (SAV). The experimental design comprises four monitoring stations with water quality probes equipped with sensors for turbidity, chlorophyll-a, fDOM and oxygen. PAR sensors were included at two stations. It is concluded that SAV reduce sediment resuspension and thus Kd and that vegetated sites exhibited higher metabolic rates compared to un-vegetated sites. Unfortunately, these conclusions are not fully supported by the experimental design and results as explained in detail below. This said, the obtained data seem to be of high quality and may be used to provide some insights into the spatial heterogeneity

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Discussion paper

and metabolic characteristics of contrasting sites within an estuary.

Specific comments: The experimental design is not well-chosen for testing the scientific hypothesis of the paper (i..e that spatiotemporal variability/gradients in well-mixed estuaries is driven by the presence of SAV). Although there are two vegetated and two un-vegetated sites, these sites differ with respect to depth, substrate, eutrophication, etc making it impossible to attribute differences between vegetated and un-vegetated sites to SAV alone. The hypothesis/aim of the study should be revised. Also, the physical/hydro-morphological/biological characteristics incl. hydrodynamic properties of the monitoring sites as well as the rationale behind the choice of the sites should be carefully described.

It is not clear how the signal from the sensors were quality assured/rinsed for outliers, off sets, drifting etc. before use. Please describe any post processing procedure of the raw signals. The max values in table 3 don't indicate a "problem" with e.g. artificial spikes (which is somewhat surprising). However, the 0 values of turbidity, chla and fDOM seems a bit unrealistic.

Time series of chlorophyll, with high concentrations during resuspension events suggest domination of suspended microphytobenthos/dead microalgae. Describe/discus if microalgae primary production is dominated by pelagic and/or benthic microalgae.

It is concluded that the presence of SAV controls the shear stress-resuspension relation (P20, L 3). However, as the physical characteristics of especially the shallow sites suggest that microphytobenthos could be abundant, this would significantly influence the shear stress-resuspension relationship as well. Hence, SAV may not be the only/main explanation for the observed seasonal trend in shear stress-resuspension relation.

Results of significance test/SD would be highly appreciated, when presenting the results for the four sites (e.g. table 3 and text). Fx. it is states that mean (over what time period?) of chla concentration at station CB11 is twice as high as for the other stations

(P10, L11-13). It would be nice to know that the observed difference is significant.

More importantly, it is concluded that vegetated sites exhibited higher metabolic rates compared to un-vegetated sites (e.g. P20, L 6). This conclusion is not supported by the results in Table 1 where the apparent difference in Pg and Rf does not seem to be significant between site CB3, CB10 and CB11. Furthermore, Pn for all stations seems to be statistically similar.

Direct PAR measurements (and derived Kd values) are only performed at the two shallow, vegetated sites. Although it is difficult to assess the quality/performance of the model from fig. 2 and 3, it seems reasonable that the model can be used to close the gabs in the time series at these stations. However, since light attenuation is dependent on e.g. particle size it seems unjustified to apply the model for stations where it is not calibrated/validated especially since the sediment at the different stations seems to differ in particle size and quality (mud vs sandy sediments and organics enrichment).

Determining the optimal sampling frequency is a science in itself and the results regarding the influence of sampling frequency (table 3) is interesting, but as presented it seems as an unfinished story that is not properly treated in the result and discussion section. This part should be either up- or down scaled/skipped.

The present study do not examine gradients, so this term should be avoided (e.g. P1, L 13). Use “patchiness” or “spatial variability” instead.

Technical correction:

Table 3: State which time period (yearly, season, other?), min, max and mean is covering. Include SD for the mean values.

Figure 6: add x-axis titles

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