

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: 6 August 2018

The manuscript “Spatiotemporal variability of light attenuation and net ecosystem metabolism in a back-barrier estuary” presents the results of a comprehensive water quality sampling program situated in Chincoteague Bay, Maryland/Virginia. The manuscript is well-written and all results are presented clearly.

There are three main concerns I have with the manuscript as it currently stands:

1. There appears to be no clear conclusion apart from the point that measuring quantities with high spatiotemporal resolution is useful – so the manuscript in its current form lacks novelty.

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For example, consider the last sentence of the Introduction, “Our conclusions highlight the importance of quantifying spatiotemporal variability in these processes, which indicate feedbacks between physical and ecological processes in marine environments that should be considered when evaluating future ecosystem response.” However, there is no explicit consideration of feedbacks in the manuscript apart from a brief mention in the Discussion.

Alternatively, consider the last sentence of the Abstract, “This study demonstrates how extensive continuous physical and biological measurements can help determine metabolic properties in a shallow estuary, including differences in metabolism and oxygen variability between SAV and phytoplankton-dominated habitats.” The first half of this sentence is a self-evident point, but regarding the second half of this sentence, there is no specific quantitative analysis in the paper comparing sites that are SAV- and phytoplankton-dominated.

2. Time series data presented in the manuscript has already been published in a technical report available online – this would be fine if there was sufficient quantitative analysis of this data (see next point), but the figures showing these time series data also do not explicitly acknowledge that this data is already published elsewhere. Most of the data presented in Figures 2-5 and 7 is identical to data presented in Figures 46, 49, 50, 52-56 of the technical report Suttles et al. (2017) cited within the manuscript. Furthermore, there appear to be other water quality stations present at the study site (see Figures 2 and 3 of Suttles et al. 2017) that measured relevant quantities during the time periods of the study whose data was not considered in the manuscript – the reasons for this also need to be addressed.
3. There is very little quantitative analysis of the results, and conclusions appear to be drawn from the presented figures without sufficient justification. Consider the first paragraph of the Results. The first sentence states that “Turbidity ranged from near zero to a maximum of over 400 NTU at site CB06 during a winter storm

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that induced waves exceeding 0.7 m (Figs. 2-5).” However, there is no indication in the manuscript (or figures) of when this winter storm took place, and no data presented for wave heights. The second sentence states that “sites CB03, CB10, and CB11 had similar statistical distributions of turbidity”, but there is no statistical analysis of turbidity present in the manuscript, only time series data.

If this manuscript were rewritten for future publication, one possible focus could be on the spectral signals shown in Figure 6 to potentially give advice to the broader scientific community regarding the temporal scales for which water quality quantities need to be measured in order to sufficiently capture their “true” values, e.g. for comparison between sites and/or time periods. Overall, the manuscript needs to go beyond the presented time series and undertake further statistical (or other relevant) analyses of these time series to reveal differences between sites. With such analysis, it may be possible from the excellent data, obtained from this monitoring program, to yield conclusions that are novel and broadly applicable to the scientific community.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-335>, 2018.