

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2022-119

Dirk Koopmans (Referee)

Referee comment on "High metabolism and periodic hypoxia associated with drifting macrophyte detritus in the shallow subtidal Baltic Sea" by Karl M. Attard et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-119-RC1>, 2022

This is valuable work on an under-studied benthic ecosystem. The manuscript presents oxygen fluxes over macrophyte (*F. vesiculosus*) detritus that accumulated in a topographical depression in shallow waters of the Baltic Sea. The manuscript is well-written and concise. The methods are clearly presented and appropriate to the goals. The primary findings are 1) that hypoxia occurs frequently in the depression and periodically in overlying water, and 2) that there is substantial detrital photosynthesis. These findings support the broader implications that 1) benthic hypoxia in shallow waters of the Baltic Sea is underestimated, and 2) the retention and export of *F. vesiculosus* carbon from coastal zones is likely greater than previously estimated.

I enjoyed this work and have minor suggestions for its improvement. My primary criticism is that the manuscript does not provide more context for metabolism of the detritus, nor for its contribution to the occurrence of shallow water hypoxia in the Baltic Sea.

On the first point, it is surprising that detrital gross primary production was so close to respiration, particularly in May and June. The authors begin the Discussion by comparing GPP to that of attached *F. vesiculosus* canopies (line 333), but it would be helpful to provide more detail. I recommend that the authors add a figure that shows how the metabolism of these detrital canopies differs from that of attached *F. vesiculosus* canopies.

On the second point, I recommend that the authors provide more perspective on the occurrence of shallow-water hypoxia in the Baltic Sea, and how *F. vesiculosus* detritus may contribute to it. Where else is shallow-water hypoxia observed? Does it naturally occur elsewhere (apart from areas of high anthropogenic impact)? Based on prior work, can one estimate how much detritus is exported from attached *F. vesiculosus* per year? Given this export and your results, what area of the topographical depressions in shallow water of the Baltic could behave as you have observed here? I acknowledge that there are complicating factors which may prohibit the authors from estimating this. For example, much of *F. vesiculosus* detritus decomposes in the intertidal zone and therefore would not

contribute to oxygen uptake in shallow depressions. Nevertheless, it would be valuable to include a discussion of knowns and unknowns.

Minor points

line 87 - two citations are used. One is relevant to the first half of the sentence, the other is relevant to the second half. I recommend separating the citations to denote the portions of the sentence that they are relevant to.

line 137 - deployments were performed on June 2017, September 2017, and May 2018, but in Figure 5 the deployments are listed as June 2017, September 2017, and June 2018.

line 139 - McGinnis instead of Mcginnis.

line 152 - "The storage correction term was defined as an average of the O₂ sensors located within and above the canopy." Figure 2(b) shows a dissolved oxygen profile within and above the canopy that is not simply an average of the O₂ sensors. Why not use this approach to also correct for storage?

lines 194-197 - Seagrass leaf length and canopy density were determined, but don't appear again in the results. Perhaps these analyses can be left out of the manuscript.

line 209-210. "The wet weight for each species was noted with 0.0001 g accuracy" is an unnecessary statement. I suggest removing it.

line 237. The sentence begins "In the upper canopy region...", but the preceding sentence already focuses on the upper layers of the canopy. I recommend replacing the quoted words with "There."

line 265 - the deployment months are listed here as June 2017, September 2017, and June 2018.

Figure 3. The symbol key lists Flow velocity 0.125 s (cm s⁻¹) and Flow velocity, 10 s (cm s⁻¹). I suggest that the word "Mean" be included for the second label.

Figure 4. Consider including PAR and rearranging the panels. Panel a could be PAR, panel b could be O₂ flux and flow velocity, then panels c and d could be the insets of O₂ flux over time.

Figure 4. It is not clear that the insets provide valuable data to the manuscript. I suggest either referencing those rates explicitly in the manuscript or removing them.

Figure 5. Again consider including PAR in the figure. It is useful to compare across seasons and to align with observed changes in flux.

Figure 6. Consider coloring the O₂ flux symbols by the time of year.

line 299 - the number of significant digits is inconsistent in this section. "...area of detritus was 2300 m², amounting to 3,832 kg dry weight..." I suggest rounding all numbers, including microfaunal abundance, to an appropriate significant digit e.g., 17259 to 17300.

line 346 - It would be useful to include a short analysis of factors that could use seasonal differences in GPP and R in the detrital canopy. GPP in particular appears smaller in September than in the earlier months.

line 356 - I believe that the reference to Figure 4 is intended to refer to Figure 3.

line 374 - "Topographical depressions with limited water exchange occupy ~1350 km² of the northern Baltic Sea" This is interesting but vague. Could the authors provide more details? What is the extent of these depressions relative to surroundings? Are these all in shallow areas? Were there other important characteristics? How was the total area quantified?