Reply on RC1
Karl Michael Attard et al.

Author comment on "Drifting macrophyte detritus triggers ‘hidden’ benthic hypoxia" by Karl Michael Attard et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-119-AC1, 2022

Reviewer 1: Dr. Dirk Koopmans

Reviewer comment: 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.

Author Response: We are grateful to Dr. Dirk Koopmans for providing a thoughtful review of our work. To provide a more detailed comparison of detritus metabolism to intact canopies, we propose (1) including an additional panel in Fig. 5 that includes daily GPP, R, and NEM of intact *F. vesiculosus* canopies from our previous work (Attard et al. 2019a), and (2) describing this panel and the comparison explicitly in a few sentences in the Discussion under section 4.1.: Detritus metabolism rates.

Reviewer comment: 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.

**Author Response:**

**Q1: Where else is shallow-water hypoxia observed?** Information on shallow-water hypoxia is generally scarce, but we have some numbers that we will include in the revision. Our key reference is the study by Virtanen et al. (2019) for the northern Baltic Sea (Gulf of Finland and Archipelago Sea). This region has a total seabed area of 12435 km² and a shallow-water area (0-5 m depth) of 2211 km². Based on their model, the total area prone to hypoxia is 1351 km² (all depths) and 16.5 km² for shallow areas < 5 m depth. Of the 461 monitoring stations in this area of the Baltic Sea that registered hypoxia, only 11 were in waters < 5 m depth. These are likely underestimates since the O2 measurements driving the models are done 1m above the seafloor. We will state this explicitly in the revision.

**Q2: Does it naturally occur elsewhere (apart from areas of high anthropogenic impact)?** Yes, O2 deplete conditions and even sulfidic conditions are often observed in association with macrophyte detritus, even in remote and pristine environments such as the high Arctic (Glud et al. 2004, cited in L32). We will state this explicitly in the revision.

**Q3: 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? In a previous study we estimated that F. vesiculosus export \( \square 0.3 \text{ kg C m}^{-2} \text{ yr}^{-1} \) (Attard et al. 2019b). Given that habitat distribution models for the area indicate a dominance of F. vesiculosus in shallow waters < 5 m depth (Virtanen et al. 2018), we have reason to believe that other topographical depressions accumulate macrophyte detritus and would likely function in a similar manner to our study site. We will state this explicitly in the revision.

**Minor points**

**Reviewer comment:** 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.

**Author Response:** OK, we will adjust accordingly.

**Reviewer comment:** 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.

**Author Response:** Thank you for catching this inconsistency. The deployment started on 31st May and ran into June. We will adjust to say May 2018.

**Reviewer comment:** line 139 - McGinnis instead of Mcginnis.

**Author Response:** OK, we will adjust accordingly.

**Reviewer comment:** 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?

**Author Response:** This is a good suggestion; we will implement accordingly.
Reviewer comment: 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.

Author Response: It is correct that seagrass leaf length does not appear. We will exclude. Seagrass density does however appear in Table 2 (Abundance per m2).

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

Author Response: It is standard procedure to report accuracy when measuring the biomass of individual animals, which is reported in Table 3. We would like to keep this if possible.

Reviewer comment: 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."

Author Response: OK, we will adjust accordingly.

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

Author Response: Thanks for catching this. Here and throughout the ms we will adjust to May 2018.

Reviewer comment: 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.

Author Response: OK, we will adjust accordingly.

Reviewer comment: 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.

Author Response: We will consider including PAR and we will rearrange panels for improved readability.

Reviewer comment: 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.

Author Response: The insets are meant to illustrate the increase in [O2] slope quantitatively by including a regression. We will add PAR as suggested above and evaluate whether these panels are still required.

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

Author Response: We will consider including PAR although we need to evaluate whether the figure will become overly busy with data.

Reviewer comment: Figure 6. Consider coloring the O2 flux symbols by the time of year.
Author Response: OK, we will color the symbols to reflect the three different field campaigns.

Reviewer comment: 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.

Author Response: OK, we will round figures as suggested.

Reviewer comment: 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.

Author Response: We will add a small table summarizing the eddy covariance deployments (duration, daily integrated seafloor PAR, water temperature). There is a clear coupling between daily GPP and R (Fig. 5) which is likely driven by changes in daily PAR. We will include an analysis of this in the revised manuscript.

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

Author Response: Thanks for catching this. Indeed, this should reference figure 3. Will correct.

Reviewer comment: 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?

Author Response: As outlined in response to the major comment above, we will include more information regarding extent of hypoxic areas for all depths and for shallow areas in this region of the Baltic Sea. We will also comment on our expectation that other shallow depressions likely function in a similar manner to our study site.