Reply on RC2
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-AC2, 2022

Reviewer 2

Reviewer Comment: The manuscript “Drifting macrophyte detritus triggers “hidden” benthic hypoxia” investigates how a detritus mat of macroalgae affects oxygen conditions along the benthos of the Baltic Sea. The authors put their observations in context of other benthic habitats in their study area. The authors also investigate the metabolism of the detritus mats at three separate occasions (2 seasons). They find that hypoxia in the bottom layer of detritus aggregations occurs whenever water velocity is low (ca. 2 cm s\(^{-1}\)), with reoxygenation of the mat happening at ca. 7 cm s\(^{-1}\). The authors then link measurements of mat metabolism with the observed fluctuations in oxygen. I would personally like to apologise to the authors for my very late review, as caused by some personal circumstances. The manuscript is clear, concise and impeccably written. My principal criticism is that, at present, the manuscript does not a good job at establishing its scientific novelty and discussing the relevance of its findings within the context of hypoxia in the Baltic sea (a well-documented and important phenomenon). The effects of macrophyte detritus mats on oxygen concentrations and benthic fauna are well documented (e.g. (Tzetlin et al. 1997; Mascart et al. 2015; Hendy et al. 2021)), including in the Baltic Sea (e.g. (Sundbäck et al. 1989; Bonsdorff 1992; Norkko et al. 2000; Berezina 2008)), so the manuscript needs to do a better job in clearly outlining its scientific contribution. The authors have a nice dataset of high-resolution measurements, which offers the opportunity to move towards a more mechanistic—albeit correlative—understanding of the drivers of hypoxia in macroalgal accumulations and shallow benthos. While the graphs presented clearly show a relationship between flow velocity and light availability, a more formal analysis of the data (even if it is just a correlation analysis cf. Fig. 6) would improve the reader’s confidence. That is important as there could be other (unmeasured) drivers that may be somewhat influencing the oxygen concentration. For instance, Fig. 2 shows no hypoxia towards the morning of Day 3 (end of the graph) despite a ~3 hr period of slow water velocity, which contrasts with the really rapid development of hypoxia in Day 3 as soon as water velocity slows. Similarly, there is no rapid recovery period (cf. Fig. 3) at night on Day 2 despite high flow. Is that related to the light conditions? Hard to tell without a more robust inspection of the relationships.

Author Response: We are grateful to Reviewer 2 for taking the time to provide a thoughtful review of our manuscript. We appreciate that our study scratches the surface of what is a complex topic and that resolving the mechanistic drivers in a causative manner would require a larger multidisciplinary effort. In short, there is much scope for further
investigation. We will endeavor to take on reviewer suggestions to improve readability. Figure 2 is an interesting example and in hindsight, it deserves more words than we allocated in Section 3.2. The reviewer rightly points out that the mean flow velocity alone does not explain the dynamics in buildup/erosion of hypoxia. In fact, from close analysis of the hydrodynamics data, we conclude that it is the prevalence of surface waves, rather than the mean flow velocity, that best explains the buildup of hypoxia. This is mentioned in our abstract (L21: “...hypoxia...terminated at the onset of wave-driven hydrodynamic mixing”) and in the discussion (L352) but unfortunately it isn't elaborated further. Our conclusion is however consistent with direct observations of canopy turbulence and mixing which suggest that surface waves are much more effective at ventilating macrophyte canopies than horizontal flow velocity (Hansen & Reidenbach 2017). Surface waves are evident in the velocity time series in Fig. 2. Looking at the first period with high flow velocity in Day 1, we can see that the variance around the mean flow velocity is quite small, suggesting minimal surface waves. The second period with high velocity in Days 2 and 3 have a much larger variance around the mean, suggesting the presence of significant surface waves. In the revision, we will elaborate on this and include an analysis of wave statistics (wave height, wave orbital velocity) to correlate with the buildup of hypoxia. Our measurements overwhelmingly show that it is the presence of surface waves rather than light or other environmental parameters that determines the buildup of hypoxic conditions in detrital canopies.

Reviewer Comment: In that context, I missed a more formal discussion of influence of sediment metabolism, salinity and the halocline on the observations, given that they are known to be important drivers of the oxygen dynamics in the Baltic. For instance, the authors also took high-resolution measurements on a nearby (~4km) sediment community, so not comparing the results with the ones from the detritus aggregation more explicitly seems like a missed opportunity. Such analysis could help the reader better understand how sediment metabolism can influence oxygen in the study area. This is important as it can help solidify the link between detritus metabolism and oxygen fluxes, which is currently not fully developed (see comment below in discussion).

Author Response: We will include more discussion on the influence of sediment metabolism, in particular the expected O2 consumption driven by the underlying sediments. We quantified this in a previous study (Attard et al. 2019a). We also performed O2 consumption measurements on F. vesiculosus fragments that we could scale up to the biomass to investigate the O2 drawdown- we will include these in the revision. The depth of the halocline is important for seasonal hypoxia but we do not think it will impact the buildup of hypoxia at our shallow site. The halocline typically is located at 30-50m depth.

Reviewer Comment: Another area that would benefit from improvement is the contextualization of the results. The Baltic Sea is well-known to be prone to hypoxia, with multiple drivers acting at different spatial scales. A better description of that system in the Introduction would help frame the importance of the study’s aims. I suggest writing a paragraph about Baltic hypoxia and the existing knowledge gaps. Additionally, further discussion and contextualization of the results beyond the study area would also improve the manuscript. How prevalent may be Fucus detrital aggregations given its cover in the Baltic? What may be their relative importance in driving hypoxia compared to the more well-studied aggregations of filamentous algae?

Author Response: We will include a better description of the Baltic Sea and hypoxia, highlighting the knowledge gaps that we aim to tackle with this study (i.e. the prevalence of periodic hypoxia in shallow waters). Reviewer 1 had similar suggestions to elaborate on how widespread shallow-water hypoxia might be. We reproduce our response to Reviewer 1 below.
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.

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 □0.3 kg C m⁻² 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. Regarding the impact of filamentous algae, it is difficult to separate out their influence because our study site contained a mixture of *F. vesiculosus* and filamentous algae (Fig. 1b), although the detrital biomass was overwhelmingly dominated by *F. vesiculosus*.

Reviewer Comment: Overall I was not convinced about the “hidden hypoxia” angle given that this is a well-documented phenomenon as the authors point out (e.g. Jørgensen 1980, see also some of the references I included) and so it is really not “hidden” at all. The reason why we don’t measure that hypoxia in monitoring programs is probably practical. I would advise on minimizing that angle in the title, intro and discussion. The bigger contribution on the manuscript is somewhere else, e.g. in the high resolution measurements and examination of oxygen drivers. If the authors decide to continue on the “hidden” hypoxia angle, I would advise on elaborating further on why does it matter that we can detect smallscale hypoxia near the sediment surface.

Author Response: We are happy to consider an alternative title for our paper, perhaps “Oxygen dynamics in accumulations of drifting macrophyte detritus”. Our decision to focus on ‘hidden hypoxia’ was in fact to challenge the modus operandi of how we measure O₂ in coastal waters. Even though Jørgensen highlighted this fact in his study more than 40 years ago, we continue to measure O₂ at some distance from the seabed, thus underestimating the true extent of coastal hypoxia. However, measuring O₂ close to the seabed is challenging and requires some technological development. The seabed is a hotspot for biodiversity and biogeochemical cycling, so the occurrence of hypoxia should be of great interest. We will highlight this more clearly in our revision.

Specific comments

Reviewer Comment: There should be a better distinction between the sections and experiments conducted, as the titles “O₂ dynamics” (section 2.2) and “O₂ fluxes” (e.g. section 2.3) are a bit confusing. To someone that is not familiar, it may seem unclear why you use oxygen sensor array in one instance and AEC in another. Please outline that better.

Author Response: We will provide more descriptive subheadings to better distinguish the sections.

Reviewer Comment: I also found it hard to know when each of the measurements were taken, and why some of the results were not included in the figures. For instance why are
only 2/3 of the measurements shown in Fig. 4? It may be valid to not include some measurements, but the reader is left wondering why if no explanation is not provided. I suggest all the figures have their date of sampling included to help better guide what set of deployments the reader is looking at (e.g. oxygen array vs AEC).

**Author Response:** Thank you for catching this. We did not include the year of measurement in Fig. 4. We will include the year in the revision. In this Figure, we only show results for June and September, because these two datasets best illustrate what we want to show in the figure, i.e. the stimulated O2 consumption due to wave-driven mixing.

**Reviewer Comment:** Ln. 135. Please outline better what is it that you want to measure with this technique and why.

**Author Response:** Eddy covariance integrates over a relatively large seafloor area (∼30 m²) and extracts fluxes without disturbing the hydrodynamics or the light, which is particularly useful when trying to understand the mechanistic drivers of O2 dynamics. In the revision we will highlight why it is useful to measure benthic O2 fluxes using eddy covariance and what we can infer from the auto-heterotroph balance of detritus canopies.

**Reviewer Comment:** Ln. 163. This is a common assumption, but studies from several systems show that Rlight may be higher than Rdark. Do we know how well this assumption prevails in macroalgae systems? Including a reference may help.

**Author Response:** We do not know of studies that are specific to macroalgae canopies, but it is well documented in macrophyte canopies such as in seagrass (Juska and Berg 2022). We will mention this as well as some older studies (Fenchel and Glud, 2000) that highlight this fact.

**Reviewer Comment:** Ln. 260. Does that correspond to a daylight or night-time period?

**Author Response:** This is a nighttime period. We will revise the figure to include hour of day instead of deployment time.

**Reviewer Comment:** Ln. 270. Personally I felt that manuscript needs to link this finding with the mat measurements better, either here in the results or discussion section.

**Author Response:** Unfortunately we do not have measures of O2 dynamics in the canopy AND eddy covariance fluxes done simultaneously. Therefore we can only infer what might be the case based on the two datasets, rather than link this quantitatively.

**Reviewer Comment:** Ln. 330. How do we know that those O2 fluxes are the result of the detritus canopy and not the photosynthetic community within the sediment? A better case needs to be presented here. Consider including measurements on bare sediment area

**Author Response:** We will include measurements performed on a nearby bare sediment area. Note however that at the detritus site, the sediment was covered by a ∼20 cm-thick canopy of degrading macrophyte detritus and was thus not exposed to sunlight. We will clarify this point in the revision.

**Reviewer Comment:** Ln. 331. This is however not the main finding of the study. The Discussion would benefit from stating more upfront what the main findings of the study area in a succinct manner. E.g. you observed hypoxia in the bottom of the mat, and you link that to mat metabolic activity combined with water flow.

**Author Response:** We will consider restructuring the Discussion although we believe this is a stylistic comment. Discussions could start with the most important finding, or they
could build up to the most important finding (we currently take the latter approach).

**Reviewer Comment:** Ln. 360-370. Personally I found this paragraph a bit out of place. It seems like discussing the consequences of the hypoxia you document in the previous paragraph for faunal communities (which you measured) would flow better here.

**Author Response:** We agree that much of this information was stated in the introduction; we will largely reduce or remove this section to focus on novel outcomes.

**Reviewer Comment:** Ln. 380. This section is quite confusing, as it is simultaneously talking about detritus aggregations, habitat structure (Ln. 380), oxygen dynamics (Ln. 384) and the effects of detritus mats on diversity (391). I suggest splitting it into different paragraphs. E.g. you can talk about the prevalence (seasonal, spatial) of Fucus detritus in the study area and the Baltic, the consequences of hypoxia for faunal communities, and the consequences of macroalgae-induced hypoxia for sediment communities in different paragraphs, as there is plenty to elaborate there on.

**Author Response:** Similarly, much of this information was included in the introduction and will be removed. Instead, we will talk about the prevalence of hypoxia and the generality of our findings.

**Reviewer Comment:** Fig. 4. It would be useful if the panels had the night-time and daytime overlayed on the deployment time axis (e.g. shaded box for night). Please consider doing that for the first panel rows of Fig. 2 and 5 as well. Also please include date of measurement.

**Author Response:** OK, will implement.