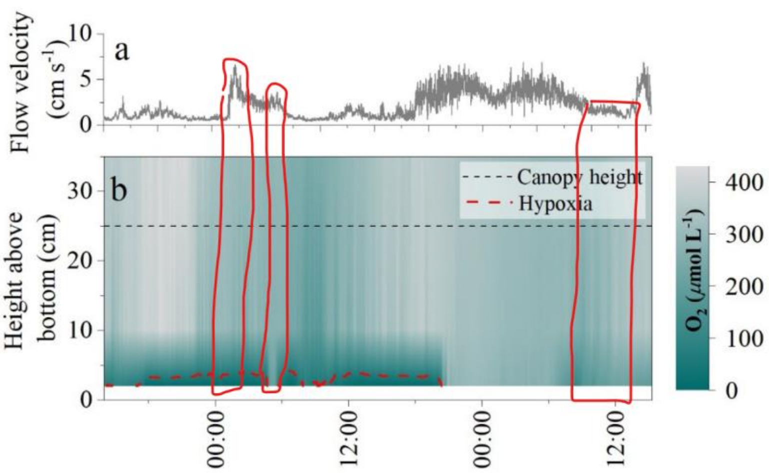
General comments

The manuscript "D*rifting 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.



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).

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?

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. Jorgensen 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 small-scale hypoxia near the sediment surface.

Specific comments

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.

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).

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

Ln. 163. This is a common assumption, but studies from several systems show that R_{light} may be higher than R_{dark}. Do we know how well this assumption prevails in macroalgae systems? Including a reference may help.

Ln. 260. Does that correspond to a daylight or night-time period?

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.

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

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.

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.

Ln. 380. This section is quite confusing, as it is simultaneously talking about detritus agregations, 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.

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 measurements