Mohr et al – Limited blue carbon potential of intertidal seagrass meadows in the Wadden Sea – a case study in a tidal basin

For: EGUSPHERE / Biogeosciences

Reply to Review by Jan Vermaat

Reviewers' comments are in black, Author replies are in blue.

I have now reviewed this interesting manuscript. I have a substantial list of minor editorial issues that I will present below, but there is also a few issues in my view that are at the moderate-to-major scale and in my view deserve attention and revision before the paper is to be published. In summary, I think that the paper (a) needs a more complete description of the two modelling components that are combined here, (b) they should choose a different parametrization for the seagrass component based on values available in the literature for their specific intertidal seagrass species rather than values from subtidal stands or different species, and (c) they need to make their validation/verification explicit. Furthermore, I have quite a list of small things that will easily improve the paper. The overall message, that we should moderate our high expectations of the potential blue carbon sequestration by intertidal Wadden Sea seagrass beds, in my view stands convincingly.

Moderate/major issues

Parametrization and description of the seagrass growth model. I have no problem with the use of just a few biomass state variables or boxes, but ....

Thank you for your detailed and thoughtful comments on our manuscript. We are pleased to hear that you found the work interesting. We appreciate the comments on the manuscript and on the seagrass model in particular and will address them in a revised version. Here are some more detailed points on how we will address the issues you raised:

(a1) But I see no good reason why canopy height data are derived from a permanently submerged *Z. noltii* bed from a Mediterranean lagoon (L135), when values for intertidal stands are readily available in the literature. We have them from SW Netherlands and Mauretania, but more importantly, Schranz & Asmus (2003, MEPS 261) provide such numbers for the Sylt-Rømø bight: 11 cm in exposed and 17 cm in sheltered systems. Also, we have shown that shoot dimensions do not change very much during the growing season, but the beds get denser and expand (Vermaat & Verhagen 1996). Finally, but why do we actually need canopy height from biomass? For estimating sediment roughness and hence have a factor for the entrainment of POC? Or for estimating the depth of the canopy for light availability?

Notice that we found that most of the photosynthesis occurred during low tide. It would be good to explain this here.

(a1) Thank you for pointing this out, since there actually was a slight oversight during the writing process of the manuscript. The formula inserted here refers to a previous formulation of the seagrass model, where the total above ground biomass was calculated instead of the single shoot biomass. We also checked the data from Schranz & Asmus (2003, MEPS 261) and found that the conversion rate from biomass to canopy height is similar to what we used: $h_{can}/B_s = 20\ m\ gC^{-1}$ (our model, from Laugier et al. (1999))

$h_{can}/B_s = 19.3 \pm 4.3m\ gC^{-1}$ (Schanz & Asmus)

The canopy height is used a) for tuning the model, since there is no biomass data available from the region, but only data for canopy height and b) when calculating the impact of the seagrass on the hydro- and morphodynamics. The seagrass parameters used for calculating the impact on the hydro- and morphodynamics are updated in the hydrodynamic model after every timestep of the seagrass model (1 week), to have the seasonality of the seagrass impact on its environment.

(a2) The function fexpAG reflects leaf loss and is set at 40% (L152). We have quantified this leaf loss during the season and came to 51% in our 1987 paper and likely even higher in our 1996 paper (75% if I now quickly subtract mean net growth rate from mean gross growth rate). As the authors say on L 151, this is an important variable for POC loss, but the paper does not present a sensitivity analysis on this aspect. Or, at least I could not trace this in the table A1 and figure A1. Table A1 does include leaf loss and shoot loss (LossN), but I failed to reconstruct how these translate to fexpAG of L 152. As an aside, this figure A1 is very hard to decipher and I suggest the authors to really make the sensitivity analysis more readable. Further, I fail to follow what is described in appendix B. Here we need a more elaborate explanation too.

(a2) f_{exp} is related to the leaf loss, but not the leaf loss itself. Leaf loss is calculated as described in Appendix A, equation A9 as a function of inundation time, day of the year (as a proxy for the collective age of the leaves in the meadow) and wind. f_{exp} is then the fraction of the lost leaf biomass that is afterwards converted to POC. The values are based on estimates of the fate of carbon originating from seagrass biomass by Duarte & Krause-Jensen (2017) and Zou et al. (2021). We will clarify this in the revised manuscript, to avoid confusion in readers.

(a3) the paper is accompanied by a more detailed description of the seagrass model in appendix A. I suggest the authors to revise the text and leave variable explanation fully to the appendix and only describe the generic principles. Particularly how and why the environmental variables are derived from the hydrodynamic model and then shunted into the seagrass model in a weekly timestep.

(a3) We agree that adding the calculation of the environmental variables from the hydrodynamic model to the model description is beneficial. And will add this to the revised manuscript.

(a4) L419 Your Ik estimate is from Kohlemeier (2016), this reference is absent in the reflist. Ic is from Olesen & Sand-Jensen (1993) and hence *for Zostera marina*. Personally I would have used readily available seasonally variable values for *Z. noltii* from Vermaat & Verhagen (1996) for intertidal Z. noltii, for photosynthesis, but also for growth. Your compensation light level Ic for growth should be plm 5 E/m2/d from our fig 3a for leaf RGR), which converts to plm 12-15 W/m2 PAR. This is not a trivial difference from the value of 9 you used, and I think this is a serious issue to consider, since *Z.noltii* is a high-light adapted plant compared to *Z,marina*. Also Leuschner & Rees (1993, AQBOT 45) present PI curves for *Z.noltii* taken on the mudflats of Sylt during emersion, and these data would have been preferable over literature values for *Z. marina*. On L465 you write that 'no definitive values are available from literature for the different parameters affecting growth of *Z. noltii'*. Obviously, I dispute this here, you could have used ours, for a starter.

(a4) Thank you for pointing out the missing references. We will make sure to recheck them.

Also thank you for the valuable insight on the light dependency of *Z. notlii* and the additional references. We will keep the value for Ic for this study, since changing it only slightly impacts the seagrass growth in the model (see Figure A1). We will also review the phrasing in L465, since we do have values for *Z. noltii*, just not for all of the parameters used in the model.

(a5) you estimate an optimal temperature for photosynthetic growth in Table A1. I assume that the word growth is sufficient, and I would prefer the use of references for *Zostera noltii* over those on *Zostera marina* that you have used. And I do think the value of 16 degrees you arrived at from your tuning is pretty low for this intertidal species. Massa et al. (2008, Hydrobiologia) show a pretty flat survival curve up to 37 degrees C, and Perez-Llorens et al (1993 Hydrobiologia 254) show for intertidal narrow-leaved *Z. noltii* that CO2 uptake is not affected measurably up to a temperature of 30 degr C.

(a5) - Leuschner and Reese (1993) found an optimal temperature for growth of Z. noltii at 20°C and a reduction in photosynthesis if the temperature decreased or increased. While considerably 16°C is lower than 20°C, it is sufficiently close to reproduce the patterns of seasonality for this study. Again, we will reevaluate the model calibration for further studies using the model, including the information we gained from the papers supplied in this review.

My main concern actually is how the model outcome is verified against what happens in the reality of the Rømø-Sylt embayment. In my interpretation the main model outcome has two important aspects (a) the seasonal growth of the seagrass, and (b) the spatial distribution of the lost leaf material across the bay. This seasonal growth is compared or validated with measurements, but the authors remain vague about the origin of these sites 'sparse in time and space', 'multiple local sites' (L199). In my view it is very important to know how much plant material is produced and where in the bay this occurs, but we cannot trace this from the paper in its current form Particularly the issue 'in space' is critical: where are these beds in the years that are simulated? Is it all the beds that are visible in Fig 1b? Figure 3 shows a seasonality for 5 years, but is this averaged across all pixels with seagrass? In the discussion (L~~1~~269) the authors mention there is a notable change in the spatial extent, but this is nowhere made explicit in a table or figure, at least not that I could figure it out.

So how is the model outcome actually verified with field observations? I only find a qualitative 'are well within the range'. With so much effort on constructing a complex model, I would have expected an observed-predicted plot, not only of standing stock or shoot density, but also of spatial extent. Maybe the latter was not possible, but then the basis of POC production and then redistribution across the bay as well as local and further away burial is unconvincing. I suggest the authors particularly clarify this issue in their revision, include the annual variation in spatial extent of the seagrass beds as well as POC transport and burial and are clear about possible weaknesses. Verification of seagrass-derived POC burial may be a trifle more difficult, but could be assesses against sediment organic matter, where hotspots of accumulation as suggested by fig 4 may well correspond to areas with higher sediment organic matter.

Concerning your comments on the spatial distribution of the seagrass biomass in the bay: The growth of seagrass is restricted to the observed meadows for each year, which are indeed shown in figure 1b. We will clarify this in the revised manuscript.

Finally on the model performance, since light availability at the canopy is such a crucial aspect, personally I would have greatly preferred to see (a) how this light climate did vary across and between years, and how this then affected the seasonal shoot density or aboveground biomass curves; and (b) how it worked through the model work flow.

The last paragraph of the introduction suggests that the model explicitly resolves the bio-physical interactions between seagrass and .. environments', and when searching for a hypothesis, research question or objective, I find that the authors 'seek to fill the gap in quantitative research of carbon sequestration potential of seagrass meadows in the Wadden Sea'. Does the paper meet these two expectations? Regarding the former, I must say I cannot find how they modelled the effect of the seagrass on the hydromorphology, I can only see a oneway influence of hydrodynamics on seagrass growth and then POC transport. I would appreciate to see how, or if, they modelled the effect of the seagrass on the hydrodynamics. Regarding the latter, I would propose a slight rephrasing including the word ‘intertidal’, for example like ‘quantify the annual fluxes and fate of carbon produced by intertidal seagrass in a Wadden Sea bight’, but then I would say the authors have reached their objective. Figure 7 gives us a complete annual budget that includes the different export terms, and in its form for me is convincing. One might wish the inclusion of other primary producers in the budget, but that was not the study objective so that is fine.

On to the feedback of the seagrass model to the hydro- and morphodynamics: There is feedback, since the seagrass parameters influencing turbulence and drag are updated in the model after every seagrass timestep. We will add additional explanation of this to the manuscript.

Minor Issues

We also appreciate the detailed comments on smaller issues with the text and will make sure to address them in a revised version of the manuscript, including updating the figures to increase their readability.

Thank you again for your very detailed review. We hope that we can address the issues raised adequately with our response.