

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 Theodor Kindeberg

Reviewers' comments are in black, [Author replies are in blue.](#)

Mohr et al. present modelling results of carbon uptake, burial and export in an intertidal seagrass system in a Wadden Sea bay. A seagrass growth model is coupled to a hydrodynamic model including particle tracking and covers four years of seagrass variability to estimate organic carbon burial rates within and outside the seagrass meadows. This combined approach encompassing advective transport, morphodynamics and C cycling provides a source-to-sink assessment of the fate of seagrass-derived organic carbon that is often lacking in the “blue carbon” literature. I commend the authors for this holistic perspective and appreciate their effort. However, I have a few concerns regarding confusing terminology, interpretation and conclusions that should be addressed and clarified before I can recommend this for publication.

[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 terminology in particular and will address them in a revised version.](#)

First, while the major strength of this model is the holistic perspective on seagrass carbon cycling, encompassing both POC burial rates within the seagrass meadows and export to adjacent ecosystems, none of these processes seem to have been validated by field data. This makes the conclusions less robust, and the lack of in situ measurements should be explicitly addressed as a caveat in the discussion. This is especially important because of the two assumptions that all POC is recalcitrant (L149) and that all POC that enters the surface sediments are sequestered long-term.

[The missing observational data is certainly a drawback of the study. We agree that it should be addressed more explicitly in the discussion.](#)

Second, I believe there is a mismatch in time scale perspectives. The relevance of longterm storage of POC for climate regulation is acknowledged in the introduction, but the model encompasses only four years. Yet, the authors make claims about long term CO₂ sequestration without any field validation. This time scale discrepancy should be addressed in the discussion and language referring to long-term sequestration should be toned down or better justified.

The comment on the long-term storage of CO₂ seems to be related to the issue of terminology raised later on in the review. We see this and will revise the terminology, both to reduce confusion that might come up for a reader and to make clear, that we do not look at the long-term processes, but rather on the annual processes that provide the basis for long-term carbon storage.

Second, the model estimates fairly low organic carbon burial rates in this system compared to global seagrass estimates, and the authors hypothesize that this could be due to strong hydrodynamics in the bay. While I do not doubt that carbon burial rates are low, I think the potential reasons could be further explored. Considering that an estimated 24% of all “carbon uptake” (I interpret this as being equivalent to the net CO₂ fixation by seagrass primary production) is buried according to the model, it does not seem that the low burial rates are due to low sedimentation rates but rather low productivity. The model estimates annual carbon uptake by the seagrass, and it would be relevant to compare this estimate rates to published rates, similar to what is done with carbon burial rates.

The comparatively low annual primary production as a reason for the low carbon burial rates is a valid remark by the reviewer and we are grateful that it is pointed out. We compared the values of seagrass primary production in our model to that of other regions and indeed our primary production is quite low compared to observations for *Z. noltei* in the Netherlands and France (Vermaat et al (1987), Pérez-Lloréns and Niell (1993), Pérez (1989)). We will add this to the discussion.

Third, the study focuses on a small geographical area which provides a high spatiotemporal resolution. However, many critical terms in the model (e.g. $f_{exp,AG}$, $f_{exp,BG}$, h_{can} , $LossN$) are based on global and/or first-order estimates from other systems with unknown transferability to this intertidal system. This is to some extent addressed with the sensitivity analysis in Appendix A, but these uncertainties should be acknowledged in the Discussion and especially the uncertainties of the export terms deserve more attention.

We will also address the uncertainties arising from transferring global values to our study regions in the discussion.

Lastly, the terminology concerning seagrass carbon cycling includes several terms describing similar things which leads to confusion. The authors mix carbon “sequestration”, “burial”, “fixation”, “storage”, “stock”, “uptake” and “drawdown” throughout the manuscript, sometimes confusing the terms. For instance, section 4.2 uses “carbon burial rate” whereas section 5.2 discusses “carbon sequestration rates”. I recommend the authors to ensure that the same process is referred to with the same term and if necessary, explicitly define terms to avoid confusion.

As stated above, we will rework the manuscript so that the terms are used consistently and it is easier for the readers to follow along.

In the following we will shortly address the some of the issues that were raised in the detailed comments. In our revised version we will go into more detail for all of the comments. We appreciate all of the comments and think that addressing them will certainly improve the manuscript.

Please see my detailed comments below.

L110-111: Is all lost biomass assumed to be POC? No remineralization, grazing or DOC component included? For instance, on L95 you state that there is “massive grazing”, could this be quantified for the study area and incorporated into the growth model?

L110-111: It is not assumed that all lost biomass is POC. Only the fraction defined by the f_{exp} terms are afterwards considered as POC. Remineralization, grazing and DOC are not explicitly considered, but the part of the biomass that is not accounted for as POC or debris (25%) is considered to be remineralized or grazed (Section 5.1, Fig. 7)

L126-129: Are the vegetation parameters here defined as constant or varying with season? On L93 you state that the seagrass is featured by a “strong annual growth cycle”, which would render the shoot density, canopy height and ultimately the drag being far from static across seasons. If there is a feedback from the SGINT model for these parameters it should be stated here. This is not entirely clear from Figure 2.

L126-129: The vegetation parameters considered in the hydrodynamic model change with the season and are an output of the seagrass growth model.

L145-147: If the study area experiences heavy grazing by birds, using this global estimate would risk overestimating the POC exported relative to production. I believe such heavy grazing is limited to local areas with conditions (e.g. water depth, migratory routes) conducive to herbivory. As far as I can tell, the paper used to tune the shoot loss rate (Laugier et al. 1999) does not incorporate loss from herbivory. Also, assuming that all produced POC ends up in bottom sediments neglects advective transport outside of the system and considering all seagrass-derived POC as recalcitrant is not valid on time scales relevant for long term carbon sequestration. It seems grazing is not considered in the loss term definition (L415-416).

L145-147: We do acknowledge that our model is simplified and that not all terms that are impacting the loss or growth of seagrass are considered. Including all terms into a model is very ambitious. The shoot loss rate $Loss_N$ is the background loss rate, that is further modified by temperature, day of the year (which can be argued to include the effects of the

grazing since grazing by the migratory birds starts towards the end of the year) and inundation time.

We do not assume that all POC ends up in the bottom sediments. POC originating from the above ground biomass is released to the water column. POC originating from the below ground biomass is added to the bottom POC pool. All POC is modeled as a sediment-class tracer in the model (as stated in L139), allowing for deposition and resuspension, and therefore also for the advection of POC to the outside of the system.

L201-205: How is the large spread between years in December compared to the tight agreement in January in Fig. 3 explained? It seems that the uncertainty of simulations increases as a function of time of year, but how is the convergence in January resolved between different years? For instance, how can canopy height in December 2010 be ≈ 0.09 m but ≈ 0.06 m in January 2011? Also, in Fig. 3b, shoot density $>13\ 000$ shoots m^{-2} is quite remarkable. Is this correct?

L201-205: The spread in the December compared to the tight agreement in January is explained by the fact that we have to reset the seagrass model at the beginning of every year for it to work, since we do not model the extent of the meadows, only the changes to biomass, but have different shapefiles for the meadows where we initialize seagrass for each year. The hydro- and morphodynamics are modeled continuously over the whole study period. We will clarify this in the model description.

Overall, many comments by the reviewer point to uncertainties that should be discussed or to sentences that are not clear and lead to questions with readers. We will incorporate these remarks into the revised manuscript.

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