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.

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.

Second, I believe there is a mismatch in time scale perspectives. The relevance of long-term 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 CO_2 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.

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 CO_2 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.

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}, $Loss_N$) 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.

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.

Please see my detailed comments below.

L35-43: Carbon storage and carbon sequestration are not the same thing and can be decoupled. Be explicit about what is referred to (e.g. stocks vs rates). For instance, Kennedy et al. 2022 assessed drivers of carbon stocks, not sequestration rates.

L87-93: Is this correct? From Figure 1 there seems to have been a substantial decline in seagrass cover since 2010/2011, especially along the southern shores? Also, it is not clear what "core area" and "core position" mean. Is it where the meadow is continuous? Most dense? Most stable? A brief explanation of the terms would help the reader to follow.

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?

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.

L131: So subtidal seagrass in Italy? A brief comment on the uncertainties due to this should be included.

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).

L165: It is not clear if this decay during transport is incorporated as an additional loss term or DIC/DOC source.

L199-200: Can the uncertainty of this be estimated? What is the monthly range or error bounds around the mean?

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?

L208: AG and BG biomass is already defined so "BG:AG ratio" can be used here for consistency and to avoid confusion.

Figure 3: It is not very clear which line represents the observation mean. Can this be made thicker or in a distinct color to clearly separate from simulated std?

L227-230: This calculation should be clearly defined in the methods. For instance, does "sedimentary carbon" encompass only surface sediments or deeper layers? This has important implications for the permanence of POC burial and if only surface layers are considered, this calculation would overestimate the POC buried on longer time scales.

L243-244: But still half that of the unvegetated tidal flat (L238). Considering that no other sources of POC are included (phytoplankton, microalgae etc), the comparison of burial in the seagrass meadows compared to the tidal flats seems unbalanced.

Figure 5: It is not clear what the "normal spread for the sequestration rates" refers to. Is this the spatial variability between grid cells? It is also a bit confusing what a median of mean annual burial rates are. Please clarify in the figure caption. Finally, use consistent terminology. The y axis says "Sequestration rate" but the caption uses "burial rates".

L268: There is no estimate of air-sea CO_2 flux in this study so I recommend refraining from saying that atmospheric CO_2 is taken up (unless it is assumed that all primary production by *Z. noltii* occurs during low tide). As far as I can tell, it is not possible to distinguish in the model between atmospheric CO_2 equilibrating with the seawater relative to DIC sources advected laterally into the system.

L277-280: Here would be a good place to add a sentence about the implications of applying this global estimate to the local intertidal system in this study. The results shown in figure B1 could be referred to here (but see my comment below regarding this figure).

L280: I suggest explicitly stating that this refers to inorganic carbon incorporated into seagrass biomass, to avoid confusion with previous discussion about import by allochthonous POC.

L289: So 24% of net primary production each year is buried in the sediments. This seems quite high and would make these seagrass meadows very efficient in comparison to global estimates (e.g. ≈14% of NPP for seagrass globally (Duarte and Cebrian, 1996; Arias-Ortiz et al. 2026). Perhaps this is worth highlighting, despite the relatively low estimates of the net sequestration rates?

L293-294: Here the grazing and remineralization is brought up. I suggest explaining this earlier in the methods section to make it clear to the reader that these processes are not neglected even though they are not explicitly incorporated in the model.

Figure 7: In my opinion, there should be an arrow for remineralization in the belowground/sediment compartment as well. Also, it is quite information heavy with a lot of text. Are both annual and total absolute rates and percentages necessary here? I suggest removing the *tC yr-1* rates. In the caption, define what black vs. grey arrows represent and what "long-term storage" is.

L313-315: Yes, this is a very good point that is often not considered in seagrass blue carbon studies. It would be interesting to know what the net carbon sequestration in the bay would be in the absence of seagrass. Is this something the model could estimate?

L321-322: I don't find this very surprising considering that the model neglects POC stemming from sources other than seagrass (e.g., marsh plant litter, phytoplankton, microalgae etc)? It could be reiterated here that the study is limited in the sources of POC considered.

L328-338: A revised global estimate has been presented by Arias-Ortiz et al. 2026. Depending on timing of publication of that study, it would be a relevant source to cite here.

L333-334: Repetition from L303-305.

L342: While this appears to be a reasonable expectation, the model estimated that 24% of carbon fixation is buried in the sediments which is quite high, suggesting that low seagrass productivity (relative to global rates) could explain the low burial rates.

L345-349: I suggest acknowledging this limitation earlier on (e.g. on lines 277-291).

L355: Indeed, but it would underestimate carbon sequestration outside of seagrass meadows as well (e.g. tidal flats).

L357: Indeed. Consider adding an arrow for this re-erosion to Figure 7.

L361-366: This discussion diverts from the story and could be removed. Also note that the studies referenced are either from other areas (e.g. carbonate-rich environments) or with other seagrass species making a generalization less applicable to the Wadden Sea.

L390: Again, that a quarter of all carbon taken up is buried in the sediments suggests a high burial efficiency which I find quite remarkable for an intertidal seagrass system in a hydrodynamically active area.

L392: On L289 you state it is 23.9% ± 2.2% percent of "seagrass-originated carbon"

L394-395: This is confusing. Is this a different form of total annual carbon uptake or why does it differ from L392 and L16?

Appendix B: Figure B1 is not clear to me, perhaps the caption and/or axes just needs to be modified. a) It is not obvious which sites on the x axis are tidal flats or seagrass meadows. b) It is not obvious why a fixed value of 90% is chosen. This seems quite extreme and it may be relevant to include smaller offsets as well to inform the uncertainty estimates of the rates presented on e.g. L280; c) The y axis says sequestration rate but the caption burial rate.

Appendix C: Only tidal regime, temperature and sediment transport seems to be "validated". Was there no validation or ground-truthing for POC export, POC burial etc? If not, this should be brought up as a caveat in the model interpretation.

Minor technical corrections:

L38: "*Zostera marina*"

L64: "integrated it into"

L76: The "resulting bay" seems odd. Perhaps just say "the bay"?

L155: "*Zostera noltii*" for consistent spelling

L183: Should it not be "two classes"?

L189: *Zostera* is the genus, *Zostera noltii* is the species

L198: "*Z. noltii*"

L230: “the beginning”

L251: “is still located”

L271: I suggest not starting a new sentence (and paragraph) with a number.

L283: Same here, consider rephrasing

L285: Repetition: “12.8 ± 5.1 % 12.8 ± 5”

L292: In Figure 7 it is 25.1 ± 9.6 % not 25.1 ± 6.38 %. Are these different uncertainty estimates? If so, specify.

L316: “therefore”

L317: Letters missing in “role in string carbon”?

L319: Repetition of “sequestration rate”. Consider rephrasing.

Literature referred to above:

Arias-Ortiz et al. 2026 : https://doi.org/10.21203/rs.3.rs-8462059/v1

Duarte and Cebrian, 1996 : https://doi.org/10.4319/lo.1996.41.8.1758; Arias-Ortiz et al. 2026 : https://doi.org/10.21203/rs.3.rs-8462059/v1