Reply on editors comment (Claire Masteller)

We thank the editor for the constructive comments that have helped us to clarify and improve our manuscript. Our responses to the specific questions/requests (in **bold & *italic***) are listed below.

Assessment:

I agree with the reviewers that while the model names and table provide the reader with some insight into the differences between model runs, it can be difficult to keep track of. I appreciate that the authors added more details to Table 3 to clarify the naming conventions of the model. This has been implemented for some, but not all of the figures. I do encourage the authors consistently implement the reviewers suggestion to add more descriptive model titles in the figures of the paper, specifically the maps of model output.

The purpose of changing the names was to give the reader a better overview of what is depicted in the respective figure. In figure 8 and 9 we think the short text is better suited in the image than writing *run_abio_no_TS_10 - acc_no_TS_10* and instead have added *run_abio_no_TS_10 - acc_no_TS_10* into the caption of the figure.

Table 1 clarifies that the parametrization comes from observational data, but it is unclear to me from my reading what observational data is used to inform model forcing. Additional details on this are necessary to indicate how storms may modify different aspects of the model.

Water elevation data was used which is indicated in supplementary material. We have added this information to Table 1.

Further, given that model values of pd, gd, ps, and gs are fundamental to the implementation of the model, providing the range of values used across model runs for these parameters would strength the contribution and methods section. The authors currently point to existing citations from other works, but providing further details on the range of expected values related to the parameterization and implementation of the model in this contribution would be helpful to a reader trying to reproduce this study.

We have extended the explanation and added a new formula to make the calculation comprehensible for the reader:

The other biomixing function p_d is calculated following Brückner et al. (2021), which is also based on the data from Cozzoli et al. (2019). Abiotic (τ_c^0) and biotic critical shear stress for erosion (τ_c^{bio}) are defined based on the respective τ_b value at which a minimal erosion rate of 25 g m-2 is reached. This is done by converting formula (3) into:

$$\tau_c = b - c \cdot log\left(\frac{a - R_{25}}{R_{25}}\right) \quad (8)$$

τ_c^0 is calculated using a_0, b_0, and c_0 which are constants for the defaunated control experiments given in Table 1 in Cozzoli et al. (2019). For τ_c^{bio} a_{bio}, b_{bio}, and c_0 are used. p_d is then calculated via:

$$p_d = \frac{\tau_c^{bio}}{\tau_c^0} \quad (9)$$

(9)

Throughout - please provide units for all variables at the first instance that they are introduced.

Units for all variables at the first instance that they are introduced are now provided in the revised version.

L160 - For equations 1 and 2. Are A and B fractional values between 0 and 1 or dimensional quantitates? For these stabilizing functions, are there more complex functions underpinning these or do A and B act purely as scalars? I see that these relationships are defined in the following sections, but a clarifying sentence/short paragraph at the end of this section and priming the reading for the progression to the next section would improve readability.

A is the abundance/number of individual and B is the biomass in mg AFDW. Both are calculated with the species abundance model. The way B is used in order to calculate the stabilizing functions is described in equation (4), (5) and (6). We forgot to explain how A is used, and an explanation is now added in the text. The description of the quantities A and B is also added in the text according to the suggestion.

L165-L170 - Is Rtot different that Er in Equation 2? I am unsure if these are the same variable or if there is some component of erosion specifically attributed to biomixing effects. Please add a note to clarify or check for consistency.

The meaning and difference between R_{tot} and E_r become clear when looking at the dimensions (added in the revised version). E_r is a rate dependent on time and R_{tot} refers to the result of measurements by Cozzoli et al. (2019), which describes the total amount of sediment eroded within a defined time span. R_{tot} is used to derive the biomixing function g_d, which is then fed into Equation 2 to calculate E_r. We have added a short explanation in the revised text:

In our model, the formulae from Cozzoli et al. (2019) are adopted to relate biomixing effect with the overall metabolic rate M_{TOT} (mW). In this study measurements of the total eroded sediment per unit area in a given time, R_{TOT} $(\frac{g}{m^2})$, were taken. Assuming that the erosion rate $(\frac{kg}{m^2 s})$ over the given time is constant it can be described by: $R_{TOT} = \frac{a}{1+exp\left(\frac{b-\tau_b}{c}\right)}$

L198 - Are the range of raw values used for these terms useful to report or provide here for reproducibility?

We have made this point more clear in order to ensure reproducibility:

To account for this seasonal variability, a multiplication factor for M_{TOT} was introduced according to a sine function with a period of 1 year, reaching the maximum value of 1.0 in summer and the minimum of 0.1 during winter.

204 - When erosion rate is used here is this related to a variable in the model? I see that gs is set to 1, and that gs modifies Er, but is this directly reflecting the statement regarding erosion rate, or is this a consequence of the assumption that in the winter months, both ps and gs are set to 1 and Er = Er0? Please clarify how these are related in the main text.

Yes, the modification of g_s and p_s directly reflect the statement regarding erosion rate and critical shear stress. We have modified the respective paragraph to make this clear.

L213 - In Equation 9 a new biomass variable has been introduced. Is this variable separate from B, or the sum of, Bindv, or consistent with these treatments? Please clarify.

B is biomass of macrobenthos in general. Since the effects of bio-accumulators is scaled with the biomass of bio-accumulators, a new symbol S was used. To avoid confusion we have now changed it to B_{acc} for consistency in use of symbol for biomass. Explanation is provided.

L206-L225 - Are the affections of accumulators and seagrass explicitly accounted for in the model framework presented in equations 1 and 2? I recognize that there are model parameterizations associated with these terms, but am unclear on whether or not these terms are accounted for in ps, gs, pd, and gs, or as separate parameterizations. Table 1 is somewhat helpful in clarifying these, but similar clarifications should be made in the text for readability. For example, a sentence clarifying that seagrass hydrodynamics are accounted for in SCHISM using an existing module would help the read follow the model implementation more clearly. While these are included in the table, also reiterating this in the sections where each element of the model are introduced would strength the methods.

The effect of accumulators is in sediment settling velocity and the effect of seagrass is in turbulence and bottom drag. They are not explicit accounted for in equation (1) and (2). We have modified the first paragraph of 3.2 to clarify this:

"Impacts of benthos on sediment are formulated through scaling functions between benthos abundance/biomass and model parameters for sediment dynamics, namely the critical shear stress for erosion τ_c (Pa), the erosion rate E_r $(\frac{kg}{m^2 s})$, the sediment settling velocity W_{sed} $(\frac{mm}{s})$ and hydrodynamic parameters for turbulence and bottom shear stress. For sediment erosion, the general approaches by Knaapen et al. (2003) for τ_c and Paarlberg et al. (2005) for τ_c and E_r are applied. An abiotic critical shear stress for erosion τ_c^0 and erosion rate E_r^0 are scaled by dimensionless biomixing functions p_d, g_d and stabilization functions p_s, g_s, respectively, which depend on abundance A (number of individuals) and biomass B (mg ash free dry weight (AFDW)) of these two functional groups:

$$\tau_c = \tau_c^0 \cdot p_d(B,A) \cdot p_s(B,A) \quad (1)$$

$$E_r = E_r^0 \cdot g_d(B,A) \cdot g_s(B,A) \quad (2)$$

Changes in hydrodynamics by the effect of seagrass are incorporated using the submerged aquatic vegetation model (SAV) of SCHISM (Zhang et al., 2016) and changes in W_{sed} by the effect of accumulators are applied according to a filter feeder ingestion rate model (US Army Corps of Engineers, 2000). Both are explained in following sections."

L302 - "lies below 20% deviations from the measurements for the majority of the stations" os a bit confusing. Rephrase for clarity.

We have rephrased this sentence and added two sentences:

"To assess the performance of the decision tree-based SAM model, the measured data were split into training and validation datasets. The training dataset was used for training the model and the validation dataset was checked against the resulting estimations of biomass and abundance. The performance of the SAM varies among the selected species. For the majority of the data points, the estimated value deviates from the measured value by less than 20% (Fig. S2, supplementary)."

L307 - missing a space between "theyears 2008…."
L411 - Typo, should be "Deposited"
L452 - Typo, should be "biomixers"

L486 - modify to "in the direct vicinity"
Figure 7 has a typo in "measurements"

Corrected according to comment.

Fig. 5, 8, - I totally missed the bar plots in the bottom right on first reading, I would recommend making these larger and labeling the axes to draw the readers attention and better connect the maps to the main channel plot. I think these are quite useful to provide context and aids in the interpretability of the model output maps.

We have increased the size of these plots and labelled the axes according to the editors' suggestion.

Fig. 7., 9 This figure has quite a bit of information in it. In terms of clarity and comparison, the author may consider adding a new figure or fifth panel that more directly compares the net changes in each of the identified regions to one another across the model types. I recognize that this is discussed in the main text of the paper, but an additional summary visual may reinforce the point that different model runs are capturing morphologic change in different regions of the study site with varying degrees of performance/variability. Perhaps a box plot of the changes of each model cell in each region, so all three model runs can be compared directly to observations in Region 1, and so on and so forth for each region?

We have added a new panel e) to both figure 7 and 9. In Figure 9 we have followed the editor's recommendation by adding a boxplot diagram. In Figure 7, we found that a boxplot diagram with a small number of possible values [-2, -1, 0, 1, 2] looked confusing since it shrinks to just a bar in some cases. Therefore we have adopted a violin plot which provides a probability distribution to better reflect the variability. References to figure 7e and 9e have been added to the main text.