Comment on bg-2023-108

Reply RC2

This study tries to improve numerical model predictions and validation of sediment fluxes between tidal marshes, channels and bays, by data assimilation of high resolution remote sensing imagery.

This topic is worthwhile and important to explore, timely and well suited for the audience of biogeosciences.

The study is of high quality and well written.

We want to thank the Reviewer for taking the time to carefully read our manuscript and providing comments to better present our work. In the following lines, we reply (in **black**) to each comment (in **blue**) and refer to the changes in the manuscript. Modifications are reported between "" and with *italicized* text. At the end, we also report all references to papers cited in the answers. We were not able to add all revisions. In these cases, we refer to the changes we will make in the manuscript instead of providing the actual modification.

Line 65: could more information been added here. I assume the authors are referring to surface concentrations of suspended solids derived from AVIRIS-NG

Yes, we refer to the **surface** sediment concentration. We modified the sentence to include this information. In addition, we added an information on the method used to derive those data: "*Jensen et al. (2019) used high resolution remote-sensing reflectance data from NASA's Airborne Visible/Infrared Imaging Spectrometer-Next Generation (AVIRIS-NG). They developed an algorithm based on a derivative-based partial least squares regression between measured total surface suspended solids and in-situ spectra, and} derived maps of total surface suspended solids in the waters of the Atchafalaya basin along the Louisiana coast (USA) from AVIRIS-NG*".

Line 95: this might be just a detail but I suggest to put species names in italic

Using italicized text is more precise, so we applied the suggested modification.

Line 125ff: how can the UAVSAR and AirSWOT distinguish between emerged vegetation or the water surface? Are there vertical errors ranges that could be reported?

In UAVSAR, in order to separate wetlands and water surface, a water mask is generated from the interferogram L1 products. AirSWOT mounts a digital camera that maps the water surface. However, in the case of the Delta-X mission the camera wasn't mounted as it requires cloud-free conditions. Since UAVSAR is better at discriminating water and wetlands, the UAVSAR water mask was used also for AirSWOT. We added two sentences specifying this: "*In flooded wetlands, the water surface is detected through the double-bounce scattering mechanism from water and vegetation (Kim et al., 2009; Wdowinski et al., 2013). To separate the water surface from the emergent wetland, a water mask was generated from the interferogram Level-1 products*". In the AirSWOT data introduction we added: "*AirSWOT uses cross-track interferometry to measure the elevation and combines it with along-track interferometry to correct for the bias due to the water motion (Goldstein and Zebker, 1987). To separate land from water surface, the same UAVSAR water mask was used for AirSWOT*".

Regarding the vertical error, there is an error file (named *err* in the Denbina et al. (2022) dataset) which contains an estimate of the vertical error for each pixel and provides a spatially-varying estimates based on the interferometric correlation. For both Delta-X campaigns, AirSWOT was validated using in-situ gauges and found a Root Mean Squared Error of 9 cm across the entire campaigns when AirSWOT was averaged over a 1 km^2 area. We added the last information in the manuscript at the end of the AirSWOT data presentation: "*Water surface elevation data were validated in both Delta-X campaigns using in-situ gauges and a root mean squared error of 9 cm was found when data were averaged on a 1 km^2 area*".

Line 144ff: is TSS surface or depth averaged concentration derived? How applicable is the calibration during different types of suspended particles during high and low flow conditions?

The TSS derived from AVIRIS-NG imagery is surface concentration. The in-situ measurements used to develop and validate the algorithms were collected in a wide range of contrasting water types in Terrebonne and Atchafalaya Bay during both Delta-X campaign, which covered high flow (during Spring 2021) and low flow (during Fall 2021) conditions. The algorithm to retrieve TSS from AVIRIS-NG performed well (Median Absolute Percent Difference 13.7% and Median bias 6.71 mg L^{-1}) across a wide range of TSS concentrations (0.1–154.5 mg L^{-1}) throughout both basins and in contrasting seasons. More detailed information on the spatial and temporal coverage of the sampling and algorithm calibration are in Fichot and Herringmeyer (2022) and Fichot and Herringmeyer (2023). We modified the paragraph related to AVIRIS-NG data and included these informations: "*Local empirical algorithms derived using in-situ measurements were used to derive TSS concentration form the Rrs(λ) in the visible/near-infrared region and generate maps of TSS from the AVIRIS-NG imagery (Gao et al., 1993; Bue et al., 2015; Jensen et al., 2019). The in-situ samples were collected in both Terrebonne and Atchafalaya basins during the Delta-X 2021 Spring and Fall campaigns in order to capture high and low flow conditions. The algorithm to retrieve TSS from AVIRIS-NG performed well (Median Absolute Percent Difference 13.7\% and Median bias 6.71 mg/l) across a wide range of TSS concentrations (0.1–154.5 mg/l) (Fichot and Herringmeyer, 2021, 2022)*".

Line 159: what was the grain size, what transport equations were used?

We set the sand median diameter at 0.14 mm. For the cohesive fraction, we defined the settling velocity and not the grain size. The sand transport is modelled using Van Rijn (2007), while the mud transport is modelled with Partheniades-Krone (Partheniades, 1965). A few of this information was already included, however a part was not indicated. Now we explicitly state the equation used. *"Thus, the calibration of the parameters refers to the properties of the mud fraction. The default transport equation of Van Rijn (2007) was used for sand". "The default Delft3D sediment transport formulation of Partheniades-Krone (Partheniades, 1965) was used for the non-cohesive fraction"*.

How was the bed initialized, 1- or multiple layers?

The bed is initiated as 1 layer. In each cell we defined a percentage of sand and mud based on Williams et al. (2006).

What was the active layer thickness?

The layer thickness was set at 5 m to ensure we did not run out of sediment to resuspend.

Was the option for mixed sediments used?

We did not use the option for mixed sediments and two fractions were modelled separately.

How was the non-cohesive/cohesive boundary defined?

The separation between sand and mud (non-cohesive and cohesive fraction) was based on a previous study in the area (Liu et al., 2018), in which they model deposition generated by Hurricane Gustav. Median sand diameter size is set to 0.14 mm, which locates the sediment in the fine sand class (using the Wentworth scale). Everything below is considered cohesive.

Line 165: is it correct to assume that for tidal channels/lakes and bays the same chezy coefficient was used? did the author try a spatial varying chezy? which wetting and drying scheme was used?

This is a very interesting point that was discussed during the development of the model. The assumption of a homogeneous Chezy coefficient holds some simplifications and initially we tried to use a spatially varying Chezy coefficient to improve the comparison with AirSWOT, especially for the smaller areas and channels within the marsh. However, we realized that for the large-scale models, results did not improve by varying the value of the friction. The limitations of the coarse grid outcompeted those due to the same friction coefficient. Namely, before considering more tuning of the friction, the model must correctly solve the flow in smaller areas and channels. In a possible future improvement of the models, using an unstructured grid could allow better representing smaller features. For instance, a separation in terms of friction could be done between ocean/main tidal channels and small tidal creeks.

The wetting and dry scheme is based on a threshold depth which was set at 1 cm. The algorithm 'activates' a cell when the water depth is positive and larger than the threshold, while the cell becomes dry wet the water depth falls below the threshold. More precisely, the algorithm uses half of the wetting threshold to avoid a "flip-flop" (a change of state in two consecutive time steps, due to oscillation generated by the algorithm). We added a sentence in subsection 2.5: "*A 1 cm was set as water depth threshold for the wetting-drying scheme*". We added the sentence here since we focus on marsh flooding only in the small-scale model.

Line 170: I assume ws is calculated from the median diameter and the sediment density I assume the parteniades krone relation is used for sediment pickup please indicate? See comment above what equation was used for sand.

Yes, settling velocity of sand is computed as function of the median diameter and density according to Van Rijn (1993), where one out of three different equations is used depending on the median diameter. We added the information in the manuscript: "*Note that in the case of non-cohesive particles, Delft3D does not require to specify a value for the settling velocity, since it is directly computed from the median diameter and density using the Van Rijn (1993) approach*".
The Partheniades-Krone formulation was used for the cohesive fraction. Please refer to the previous answer for more details and related modification to the manuscript.

Line 175: similar comment as above why was only one muddy sediment class in the bed considered? For instance consolidated clay lenses can possess high crit. bss.,, .. did something like that occur? how well was the initial stratigraphy incorporated in the model?

We acknowledge that within our mud class multiple subclasses are present, thus our separation represents a simplification of the real conditions. The main reasons we opted for a two-class separation, is that the only robust dataset we have to represent the bottom (Williams et al., 2006) provides sampling points were most of the time this is the only separation. As pointed out in an earlier comment, the initial stratigraphy is based on this dataset.

Line 185: is the marsh platform chezy also representing vegetation?

In this case the Chezy coefficient does not account for vegetation but only the bottom of the marsh. The 35 $m^{1/2}s^{-1}$ is a typical value used to represent bottom roughness in modelling studies (e.g. Zhang et al., 2018; Passeri et al., 2018). In order to include vegetation a more correct value would range between 10 and 20 $m^{1/2}s^{-1}$ (e.g. Augustijn et al., 2008; Stark et al., 2015) or implement formulations such as Baptist (2005). In this case, our objective was to calibrate elevation and not friction. Calibrating only friction instead of wetland elevation would have led to unrealistic spatial distribution and values of Chezy (Zhang et al., 2022). Indeed, the authors suggest to first calibrate boundary conditions and elevation. Only after these steps, a calibration of the friction would provide more realistic values of friction. For this reason, we simply set an initial and homogeneous friction coefficient. Note that, the calibration of the elevation inherently contains information of vegetation, however, when Zhang et al. (2022) ran a sensitivity analysis on marsh Chezy coefficient found non-significant variation of model performance for all Chezy values (range 8-40 $m^{1/2}s^{-1}$).

Line 199: what kind of data was provided.. suspended sediment concentration,..?

Yes, it is suspended sediment concentration data. We were not precise, so we add the ‘concentration’ term in the sentence.

Line 205: was the initial sediment distribution uniform...? was a spinup for the bed tested?

Yes, the Atchafalaya model initial sediment distribution on the bed was taken uniform and it is based on the usSEABED database. In this case we have fewer points compared to Terrebonne. A spin-up of the bed is run for about one year with a morphological speeding factor of 50 to reach the equilibrium state. During this process, the bed level is kept fixed, and the bed fraction changes to adapt to the hydrodynamics. The bed composition after the spin-up process becomes more realistic and coarser fractions appear in areas with stronger flow shear stress. We followed an approach consistent with previous studies such as van der Wegen et al. (2011) and Zhang et al. (2020). We have added this information in the subsection 2.6: “*For sediment transport, three sediment types composing the bottom: sand, silt, and mud were considered. The initial sediment distribution was derived from the usSEABED database and set uniform at 22\% sand, 39\% silt, and 39\% clay. A bed spin-up process of one year was run with a morphological speeding factor of 50 to reach the equilibrium state. During this process, the bed level was kept fixed, and the bed fraction changed to adapt to the hydrodynamics. The bed composition after this spin-up process became more realistic and the coarser fractions appeared in areas with stronger flow shear stress (see similar approaches in van der Wegen et al. (2011) and Zhang et al. (2020))*”.

Line 215: see comment above why was no spatial tunning test? this could also improve fig.5?

Regarding the spatial tuning of the friction for Figure 5, first, it is important to mention that the area in dark blue colour on the left side of the flight line is affected by the edge effect. The primary instrument of AirSWOT is the interferometric synthetic aperture radar called KaSPAR. The KaSPAR outer swath mode has incident angle between 4 and 25°. This means that at the edges of this range we find the largest error (Denbina et al., 2019). At the edges the water surface elevation might not be correct. This is something we did not mention in the discussion. We will add a sentence to provide the information to the reader. Another point regarding the friction, is that the friction is typically defined for broad classes, in particular for areas that are constantly underwater such as channels. A reasonable change in friction coefficient (meaning changing the values to physically sound values) would not particularly change the water levels.

Line 240: is the RMSE calculated over an entire M2 tide or only during the time-slice the picture was taken?

The simulations were run over the entire Delta-X Spring 2021 campaign in order to check for temporal performance of the models. For simplicity, in the paper we mention only the Terrebonne model. The RMSE we refer here is computed considering all measured and modelled water levels in 13 water level gauge of site of the CRMS network (see Table S1). This value is not related to the RMSE used with the spatial images. The last one quantifies the goodness for every single comparison.

Fig.7: although the model result are very impressive, the predicted error in SSC is still between 30% - 50% of the measured range, for what conditions in the tide is this representative, is this the best or worst case?

This flight line was collected on 05 April 2021 at 19:57 UTC. This is approximately one hour after low tide when the tide is rising. The choice of the flight line was mostly dictated by the quality of the retrieved TSS. Cloud conditions were optimal (high quality flight line) and in the area covered by the flight line, there is one of the sampling points used to calibrate the algorithm used to retrieve TSS from AVIRIS-NG. Thus, this is one of the best cases we could show of all the available flight lines.

I think fig.7 could be improved since open water or marsh area are difficult to distinguish? If I interpret the results correctly the biggest error seem to appear in shallow areas?

We have improved Figure 7 by making it larger and more readable. The second question could be another interesting point to add to the results. To give a more precise answer we will try to plot the error as function of the depth and see if it increases as depth decreases.

Line 295: was a finer mesh tested,.. was an unstructured grid tested?

In very preliminary test we tested a 50 m grid, which did not provide significant improvement but only enhanced computational costs. We did not implement an unstructured grid. This is a limitation of the model, which can reproduce water levels in the wider open areas but struggles to resolve smaller channels. This limitation can be partly addressed by switching to an unstructured grid. It has also to be noted that the unstructured grid would have limitations. Many small channels and creeks are about 1-2 m wide. Such a small size would be challenging also for an unstructured grid.

Line 305: see comment above,.. very low water levels and gradients,.. will make the mini. water depth and flooding and drying scheme relatively important?

The reviewer is correct. As we mentioned in a previous answer the wetting-drying scheme is based on a threshold depth, thus the minimal water depth is very important. Delft3D-FLOW uses an element removal algorithm. For instance, the FVCOM model, which is employed by the Deb et al. (2023) paper introduced in the next question, uses a thin film algorithm (Medeiros and Hagen, 2013). Using a different wetting and drying method would affect the final result, thus it is important to provide the information in the methods section. It is worth noting that, this observation is only valid for the marsh area that are undergo flooding and drying, therefore only the application of UAVSAR. The other applications are related to channels and bays which are always underwater (no drying involved).

Line 320: how did the marsh microtopography influence the wetting and especially drying of the interior, i.e. Deb 2023,.. used an additional porosity to limit ponding caused by submesh channels?

As we mention in the manuscript, due to the very small tidal range, the topography of the marsh plays a crucial role in the wetting and drying. The suggested paper introduces to a clever way to deal with submesh channels, which is a problem also in our case. The authors modified FVCOM, which is very different from the structured grid version of Delft3D we used. As we said a few answers earlier, this is a limit of the models we developed. The problem of artificial ponding caused by submesh channels that are not solved is indeed a limitation of the methods used to correct the bathymetry. The method depends on the ability of the model to solve the water fluxes. Therefore, if some areas are not well solved, it could be that the topography is changed even if it is not necessary. For instance, let's suppose there is a small channel that the 10 m mesh cannot represent. During falling tides, the areas near the channel drain faster, hence, they generate a larger water level change compared to internal areas. Since the model cannot solve the channel, the procedure will deepen the topography to allow water to flow and match the water level change, even if the topography is correct. We added this discussion by leveraging on some RTK measurements that we used to validate the calibrated topography: "*this iterative procedure might introduce errors in areas where the topography is correct. This effect can be noted for the three points before mentioned where the marsh is deepened. The methodology depends on how well the model is able to solve all channels. In this particular case, the points are located in proximity to a narrow channel with a 1.5-2 m cross section. The 10 m resolution represents a limitation because features such as channels and levees that are smaller than 10 m cannot be captured by the mesh. Yet, they affect water flow. In this example, we used a UAVSAR captured during falling tide, and in this phase, areas of the marsh close to these channels drains water faster than areas located internally. Since the model does not capture these channels, the method tries to compensate by lowering the marsh to achieve the measured water level change even if the elevation is correct*".

Line 370: it is unclear how the davg sediment concentration predicted by D3d was correct to compare to the remote sensing product.

This is an important point that was also raised by the other reviewers, and we agree that the current version of the paper does not clearly show it. We will perform a second calibration using in-situ measurements and provide a validation of the results using a second period (Delta-X Fall campaign).

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