Response to interactive comment by Anonymous Referee #1 on "Cohesive and mixed sediment in the Regional Ocean Modeling System (ROMS v3.6) implemented in the Coupled Ocean Atmosphere Wave Sediment-Transport Modeling System (COAWST r1179)" by Christopher R. Sherwood et al. Comment received 20 December, 2017.

The authors thank Anonymous Referee #1 for detailed and insightful comments on our manuscript. Here, we respond to those comments and indicate changes we have made in the manuscript to address them. Comments are reproduced in ***bold+italics***; our response is in plain text.

1. Does the paper address relevant scientific modelling questions within the scope of GMD? Does the paper present a model, advances in modelling science, or a modelling protocol that is suitable for addressing relevant scientific questions within the scope of EGU?

The authors extended an existing model for regional-scale coastal sediment transport and morphodynamics by implementing a number of previously developed routines that account for cohesive sediment and biogemorphology effects. The upgraded model is most likely of interest to both academics and engineers working in the coastal community.

2. Does the paper present novel concepts, ideas, tools, or data?

The present study does not present completely new model concepts, but instead, it combines existing model formulations that were developed by the same authors in preceding studies (Warner et al., 2008; Rinehimer et al., 2008; Verney et al., 2011). This leads to an upgraded version of the ROMS model, which is considered a novel tool that is worthy of publication.

3. Does the paper represent a sufficiently substantial advance in modelling science?

Yes.

4. Are the methods and assumptions valid and clearly outlined?

The implemented methods have been described in preceding studies and seem valid. However, some components in the model and underlying assumptions require additional clarification, see my specific remarks below.

5. Are the results sufficient to support the interpretations and conclusions?

The authors present results of a number of idealized "demonstration cases" and a realistic application. These cases are generally interesting and the results support the conclusions. Specific remarks regarding the simulations and the interpretations of results are listed below.

6. Is the description sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? In the case of model description papers, it should in theory be possible for an independent scientist to construct a model that, while not necessarily numerically identical, will produce scientifically equivalent results. Model development papers should be similarly reproducible. For MIP and benchmarking papers, it should be possible for the protocol to be precisely reproduced for an independent model. Descriptions of numerical advances should be precisely reproducible.

The explanations are at some points rather short, and for a full understanding of the methodology (e.g. equations and numerical implementation) the reader has to turn to preceding papers by these authors and to information contained in the Supplement. I appreciate, however, that a journal format may not allow to fully explain all the details. Given that the numeric code is available to anyone, and that the community is explicitly invited to use the code, I expect the present work to be fully reproducible.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes.

8. Does the title clearly reflect the contents of the paper? The model name and number should be included in papers that deal with only one model.

Yes.

9. Does the abstract provide a concise and complete summary?

Yes.

10. Is the overall presentation well structured and clear?

Yes.

11. Is the language fluent and precise?

The paper is well-written in fluent English.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Yes, see specific comments below.

14. Are the number and quality of references appropriate?

Yes.

15. Is the amount and quality of supplementary material appropriate? For model description papers, authors are strongly encouraged to submit supplementary material containing the model code and a user manual. For development, technical, and benchmarking papers, the submission of code to perform calculations described in the text is strongly encouraged.

The 27-page Supplement provides details on the implemented methodology, including a description of the main equations. The code is not directly provided but is available upon request.

Thank you for this comprehensive review.

Specific major comments

1. Given that one of the model goals is to simulate morphologic change (Line 98), I am surprised that the realistic application of the model to the York River Estuary does not address the morphologic evolution at all. Is the model also capable of accurately simulating longer-term morphologic changes in a complex environment such as an estuary? If the authors were to run the model for a longer simulation time (say a few years), would the model reproduce a reliable evolution of the main geomorphologic features (banks, creeks, shoals, …) of the estuary? To me this is a key issue in trusting the model's performance, and results or a general discussion on this issue are essential.

It would also be interesting to see how the modeled morphology would differ for simulations with the present, upgraded model, relative to simulations with the original model by Warner et al. (2008).

We agree that validation of the model for long simulations of geomorphological evolution is needed. And we admit that it will be a challenge to match observations of geomorphological change in cohesive environments, and can't affirm that the model will reliably reproduce changes in banks, creeks, or shoals. But, as we responded to Referee #2, validation of each component of the model would substantially expand the scope of an already lengthy paper. Comparisons of each component of the model with field data would require introduction of the observations and analysis of the inevitable discrepancies between the model and data. The goal of the paper is to describe the modeling methods, and we hope that our demonstrations of potential applications, which produce plausible results, provide sufficient guidance and incentive for others to apply and evaluate the model. We look forward to doing so ourselves. We have not changed the manuscript to address this comment.

2. A topic that is overlooked, or at least not considered in the manuscript, is bedload transport - apart from a general notion that the stratigraphy is relevant for bedload transport (L.207). This is rather confusing and I believe the following topics should be addressed:

A. Is size-selective bedload transport included at all? If yes, which model is used?

B. How does the bedload transport depend on the particle size distribution in e.g. the active layer?

C. How is the critical bed shear stress for bedload determined? Is the applied method consistent with the methodology proposed for the erosion rate in Section 2.4?

A. Yes. The CSTMS bedload transport equations included in ROMS (Warner et al., 2008) are available and suitable for transporting the non-cohesive components in a mixed bed simulation. There are presently two options: the Meyer-Peter Mueller equation, or the Soulsby equations that include asymmetric transport by waves. The transport rates are size dependent, as discussed below.

B. These equations use the user-specified particle critical shear stress for erosion for each size class, and act on any non-cohesive classes present in the top (active) layer when Tb exceeds Tcrit for that size class AND Tau_b > Tau_cb when mixed sediment is present. In other words, a sand grain embedded in a cohesive bed will not move unless the bed stress is both greater than the bulk critical shear stress of the bed and the particle critical shear stress needed to mobilize the sand grain. We assume that cohesive sediment does not undergo bedload transport; eroded cohesive material goes directly into suspension.

C. The critical bed shear stress for bedload in a mixed bed is the critical particle shear stress computed from, for example, a Shields relationship. However, the material will not undergo bedload transport unless the bulk critical shear stress for the bed (as described in Section 2.5 [now 2.6]) is exceeded. The erosion rate (flux from the bed into suspension) is governed by the greater of the two critical shear stress values. We don't think there is inconsistency in this approach, but it does assume that the presence of cohesive sediment does not affect the bedload transport rates of available non-cohesive sediment.

Text has been added to the end of Section 2.5 [now 2.6] as follows:

"Non-cohesive sediment classes are subject to bedload transport when the bottom stress exceeds both the bulk critical shear stress of the top (active) layer and the particle critical shear stress for that class. In these cases, the transport-rate equations still calculate bedload transport based on excess shear stress associated with the non-cohesive particle critical shear stress, as described in Warner et al (2008). Cohesive classes are not subject to bedload transport; if the bulk critical shear stress of the bed is exceeded, we assume they will go directly into suspension."

We thank the reviewer for bringing up this issue, because it led us to an error in the code that will be fixed in the release accompanying the final manuscript.

Minor comments

Section 2: While a section is devoted to the flux into the bed (2.2.1), the erosive flux from the bed into suspension is not described at all. The method and equations used to calculate the erosive flux should be added.

We agree and have rearranged this section and included a new section describing fluxed out of the bed, including the equation for erosive flux. See also our response to referee #2.

L198-201: It is not instantly clear how the floc size changes in the bed. Deflocculation (L.199) suggests (to me) that flocs degrade to loose sediment particles, but this appears to be in contrast with the preceding statement ("flocs erode as denser, more angular aggregates"). Reading further (and checking the Appendix), I understand that the cohesive size classes tend to an equilibrium distribution, which means that the reverse may also happen: loose clay/silt grains that form aggregates in the bed. Therefore I believe the term "deflocculation" is not well-chosen for this process.

We agree that "deflocculation" is not the correct term, because the process can go either way. We have changed it to "floc evolution in the bed". However, when larger, less-dense flocs are converted in the bed to smaller, more-dense flocs, they will be available to erode as denser particles…somewhat akin to the observed. We have changed the text in Section 2.2.2 and elsewhere to address this comment, but have not changed the CPP term DEFLOC used in the model code to enable this process.

L206-221: What happens when the bed is emerged? Are processes like shrinking/swelling accounted for in the bed stratigraphy module, or can these be added in future? Drained clay soils will become more compacted, which is accounted for in the empirical method for the critical bed shear stress. However, are these processes also considered relevant for the determination of the bed layers?

This is an important question that we have not addressed in the model. We agree that, for accurate representation of intertidal processes, it might be important to account for changes in erodibility by drying (or wetting by rainfall) during low tide. In the current version of the model, layer thickness is related to bulk porosity, but porosity does not change dynamically with compaction…only erodibility is affected. A more process-based model of compaction could be implemented without adding any state variables, but is not included in this version. We have changed the text in the discussion to list this and several processes that are not included in the model.

Section 2.4 The method to quantify tau_cb is rather crude. Could the approach be somehow improved by taking the information of the floc size distribution in the bed (Section 2.2) into account? Any reflection and/or suggestions to improve this approach would be useful.

We agree that the method for setting tau_cb is crude, although we prefer the term "heuristic". A process-based mechanism that relates sediment particle properties (size, density, shape, organic content,…) and measurable geotechnical properties (bulk density, porosity, permeability, shear strength…) would be preferred. However, the approach we have taken can be related to field measurements (e.g., erosion-chamber measurements), so there is some guidance available. The approach is also easily modified when appropriate formulations are accepted in the community.

L269: The explanations related to P_c are difficult to follow. Insertion of equation S29 from the Supplement would help understanding this section.

We agree. We have added Eqn. S29 to the Mixed Sediment section as Eqn. 6.

Section 3: The demonstration cases in Sections 3.1 and 3.2 are very interesting and insightful.

L318-325: More explanation regarding the Verney et al. (2011) experiment would be useful. For instance, what is the time of one full cycle in the experiment? Is the dip in the measurements around t = 400 min due to periodicity in velocity forcing, and why doesn't the model reproduce this dip?

Referee #2 has also commented on this. We have changed the text to clarify the model setup, and to note that the dip in measured grain diameter may have been caused by settling, which was not included in the model simulation.

L330 introduces the aggregation/collision parameter alfa and break-up/fragmentation parameter beta. Overlooking all test cases in Section 3, alfa varies by a factor 5 and beta by a factor 10. Results appear to be quite sensitive (see e.g. Fig. 3b-c) to the values for alfa and beta. How do values for alfa and beta relate to the physical properties of a cohesive mixture? And how can users determine the optimum value for these parameters? To what extent are the values used for the simulations in Fig.3b accurate (beta <0.02), as they deviate strongly from beta values for the other simulations in the manuscript?

The values of alpha and beta vary substantially in the different simulations. The rates are adjusted to reproduce the observed (or modeled) data. Ultimately, the magnitudes of alpha and beta are less important than the ratio of alpha/beta, because the ratio defines the relative effectiveness of the competing processes. That ratio does not vary as much between the experiments. More observations are needed to adequately constrain these rates. As of now, user must set the rates in the model to match available floc data. We have not changed the text in response to this comment.

L430: The active layer is defined as the upper-most layer (L222) which I interpret as being a single grid cell. Consequently I find the explanation in L430 somewhat confusing ("the active layer … extended 2 cm below the surface") given that one grid cell is 1 mm. Can the active layer comprise multiple cells/layers that erode at once, or is the 2cm erosion explained by a stepwise removal of the top "active" layer in 20 time steps?

The active layer is a single layer at the top. The thickness is determined at each time step according to Harris and Wiberg (1997). If the new thickness increases, material from underlying layers is assimilated; if the new active layer is thinner than it was in the previous time step, it is split into a top, active layer, and an underlying layer. Thickening and thinning of the active layer, in the absence of erosion or deposition, can homogenize the bed down to the depth associated with the thickest active layer. The details of this are described in Section 2.3 of the Supplement, but we have modified the text near L430 to clarify, as follows:

"The first, larger stress event (maximum = 1 Pa; Figure 5b), eroded 1.2 cm of bed, and expanded the active layer to a thickness of 0.8 cm, so the bed was disturbed to a depth of 2 cm. Expansion of the active layer homogenized enough layers to provide 0.8 cm of sediment, making fine sediment available for resuspension. The finer fractions dominated the suspended sediment in the water column, which contained only a small fraction of the coarsest sand (Figure 5a). When the stress subsided, coarser sediment deposited first, while finer material remained suspended, producing thin layers of graded bedding above the 2-cm limit of initial disturbance (Figure 5d)."

L460 "compare Figures 6c, d": I understand what I should be seeing, but the differences between the curves are too small to detect them by eye. Perhaps the period with high bed-stress should be extended to make the point.

We agree that the swelling is imperceptible. Real-world swelling time scales are quite long, so the effect of the swelling is minimal over the simulated time. We plan to run this case for a longer period and modify the figure to clarify this.

Fig. 3a: what do the error bars depict? 95% C-I, or +/- 1*st.dev?

Per text in Verney et al (2011), these represent +/- one standard deviation about the mean diameter D. We have modified the caption for Figure 3 to note this.

Technical corrections

L78-79: "that that"

Fixed.

L104: "seagrass growth model" ⍰ models?

Corrected.

L335 full stop missing at end of sentence.

Added.

L513 last sentence refers to Figure 8a, but no information on the grain size is contained in this figure. Consequently also the title of Fig. 8a is incorrect.

We agree this is confusing. This is referring to the simulation without floc dynamics, in which all of the sediment in suspension is in the 37-um size class. The text has been changed to read: "No floc dynamics were included, so all of the suspended material depicted in Figure 8a was in the 37-μm class."

The caption to Figure 8 has been changed to read: “Figure 8. Comparison of estuarine turbidity maxima simulations with and without floc dynamics. a) Two-dimensional (along-estuary and vertical) snapshot of suspended particle concentrations (shaded) without floc dynamics near the end of flood tide. All of the suspended material was in the 37-µm class. b) Snapshot of suspended particle concentrations at the same time in the simulation, but with simulated floc dynamics (shading), overlain by contours of mean particle diameters. c) Along-estuary profiles of bed elevations for simulations without floc dynamics (red) and with floc dynamics (black) at the peak of flood tide (solid lines) and at post-flood slack tide (dashed lines). d) Along-estuary profiles of mean particle diameter in the top layer of the seabed, using the same notation as (c).”