Review "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 Sherwood et al., GMD Discussions

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.

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).

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?

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.

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.

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?

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.

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

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?

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?

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?

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.

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

Technical corrections

L78-79: "that that"

L104: "seagrass growth model" → model<u>s</u>?

L335 full stop missing at end of sentence.

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.