Response to interactive comment by Anonymous Referee #2 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 24 January 2018.

The authors thank Anonymous Referee #2 for thoughtful and helpful 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.

The authors present the implementation of a cohesive and mixed sediment module within the COAWST (ROMS based system). They provide a thorough and extensive framework that includes floc model, stratigraphy and bed mixing, critical stress for erosion of cohesive sediment. None of the individual components is particularly novel in isolation, but the overall model combining all aspects does present a significant advance in coastal sediment transport modelling. The manuscript is well written and I enjoyed reading it.

Thank you for these complimentary words. We agree that none of the components is novel in isolation, but hope that we have constructed a useful modeling framework.

There are a few issues that would need to be addressed in a revision.

The most important issue is that it is not clear how the floc model is combined with the vertical ROMS grid and vertical sediment fluxes (turbulent suspension and settling) to determine suspensions of cohesive sediments. Are these actually included (the steady state test suggests yes but the comparison to Verney (2011) no)? The key discrepancy in the model-data comparison in figure 3a at t=400 min corresponds to a settling stage. In Verney et al. (2011), the settling dip was not reproduced either as particle deposition was not allowed in the 0D model. Is the same explanation also valid here?

The floc model is a zero-dimensional model that is locally integrated over the baroclinic time step, from initial to final conditions, in every cell of the ROMS model. After the floc populations are updated, the normal settling, advection, and diffusion routines in ROMS are advanced, with flux boundary conditions at the bed (erosion or deposition) and zero-flux conditions at the surface. This transport generally changes the floc populations in model cells, providing new initial population conditions for the next time step.

The steady-state test (Fig. 4) is a fully three-dimensional implementation, but the horizontal aspect of the grid is small (5 cells…just enough to accommodate the templates of the finite-difference formulations) and lateral periodic boundary conditions are applied, so that anything advected out of the domain re-enters on the upstream side. Therefore, it is effectively a one-dimensional (vertical) simulation. To reproduce the results of Verney et al. (2011), we set the settling velocities of all floc classes to zero and imposed the turbulent shear parameter, so that the simulation is effectively zero-dimensional with constant suspended mass, and the only active process in the model is the floc dynamics. Thus, our implementation has the same shortcomings as the Verney (2011) implementation, in that we cannot assess changes that might be caused by settling.

Simulations with advection, diffusion and settling are included in the other experiments of the paper.

We have added text in Sections 2.2, 3.1.1, and 3.1.3 for clarification.

Another weakness is that, even though the manuscript includes a number of test cases, it looks to me that there is a lack of validation. Only the floc model is validated against measurements and there is no validation against field observations, especially for cohesive suspended sediments. This is somewhat frustrating and looks like a missed opportunity as LISST instruments are now relatively commonly deployed in the field. Since they measure concentrations for a number of floc size classes, they would appear to be well suited to provide datasets for validation and model-observation comparisons.

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. We have collected a LISST dataset similar to that suggested by the Referee (Sherwood et al., 2012. USGS Open-File Report 1178, https://pubs.usgs.gov/of/2012/1178/title_page.html) and we plan to compare it against the model. The final section of our manuscript provides some comparison of the cohesive bed component with real-world observations. Otherwise, we hope that our demonstrations that model components work and produce plausible results provides sufficient guidance and incentive for others to apply and evaluate the model. The goal of the paper is methodological and we demonstrate the potential applications of the newly implemented routines. We have not changed the manuscript to address this comment.

Given that the new algorithms are incorporated in COAWST, I am wondering about coupling and/or compatibility with the wave module(s). While a full test of this may be outside of the scope of the paper, I think discussing this point would strengthen the manuscript.

The Referee is correct in noting that waves are closely coupled in the COAWST system, which allows two-way coupling between ROMS and either WaveWatch III or SWAN. Within ROMS, waves have several effects: a) wave-induced momentum fluxes (implemented as either vortex forcing or radiation stresses) drive circulation; b) wave breaking affects near-surface turbulence; and c) wave- and current-combined bottom stresses affect sediment resuspension and near-bed turbulence. All of these, especially the last, have direct implications for cohesive sediment processes. However, ROMS does not have a stress-strain relationship suitable for simulating the visco-elastic behavior of very high concentrations of mud. We have added text at the beginning of the Discussion to address this comment, as follows: "The improvements were implemented in the COAWST version of ROMS, which provides a framework for realistic two-way nested models with forcing from meteorology (WRF; Michalakes et al., 2001) and waves (either SWAN: Booj et al., 1999; or WaveWatch III; Tolman et al., 2014). Waves, in particular, play an important role in cohesive sediment dynamics through wave-enhanced bottom shear stresses, wave-induced near-bottom turbulence, and wave-induced nearshore circulation, but wave-induced fluid-mud layer processes are not represented."

Specific comments:

Section 2.2.1: I'm not sure whether this is the best place to present fluxes into the bed. The alternative (which probably would be my preference) is to combine with erosion into a "bed water column exchange" section.

This is a good suggestion, and we have re-arranged the paper to address it. We have added heading "2.3 Bed – Water Column Exchange" with subsection "2.3.1. Fluxes into the bed – Critical shear stress for deposition" (with the contents of previous Section 2.2.1) and a new subsection "2.3.2. Fluxes out of the bed – Resuspension" which includes the erosion rate equation.

Previous section "2.3.2. Changes in floc size distribution within the bed" has been moved up as Section 2.2.3. We thank the reviewer for helping make this section clearer and more readable.

Figure 3a,b: It would be helpful to also have the temporal evolution of G shown. Since the authors include the modelling results of Verney et al. (2011), it would be useful to explain the reason for the different model results during the first aggregation stage (initial distribution), instead of relying on the (initial distribution), instead of relying on the reader checking in Verney et al. (2011).

We agree. We have added time-dependent curves for G to Fig 3 a in the revised manuscript.

Section 2.6: The new modules are added to the existing sediment transport model in ROMS (Warner et al., 2008) and in COAWST, which includes waves. The presence of bedforms and waves may induce pressure gradients at the sediment bed, which would in turn induce interstitial porewater flow in the bed. This process can entrain fine particles into a coarser sediment bed (e.g., Huettel et al., 1996, Limnol. Oceanogr.,41(2), 1996, 309-322). It would be welcome for the authors to comment on this process and its inclusion (or not) in the present framework.

We agree this process might be important, especially for biogeochemical constituents. It is not represented in this version of ROMS because small scale bottom topography is not resolved and our version of ROMS is not yet coupled with a groundwater transport model, and we have not explored a sub-grid scale parameterization of this process. We have added to the discussion a short list of processes that are not addressed in the model, as follows: "However, not all of the processes associated with cohesive or mixed sediment have been included. For example, fluid muds and non-Newtonian flows are not represented (e.g., Mehta, 1991; 2014), nor is flow-induced infiltration of fine material into a porous bed (Huettel et al., 1999). Changes to the erodibility of mud that has been exposed at low tide (e.g., Paterson et al, 1990; Pilditch et al., 2008) or affected by flora or fauna (e.g., de Boer, 1981; de Deckere et al., 2001) are not considered."

Figure 4: there appears to be a "kink" in the concentration for one specific profile (3560 microns?). What is the cause?

The model solution becomes very sensitive, especially for larger particles, when both C and G are high, so it produces instabilities. We are not sure if these are real, or numerical artifacts, but they only occur under conditions with very high concentrations and turbulent shear.

Figure 8: Caption should include details on what the different panels (a, b, c, d) show.

We agree, and are not sure where those details went! We changed the caption to read as follows: "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. 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). The model was initialized with a uniform suspended-sediment concentration of 0.1 kg/m3 in the 37-µm class."

Technical corrections:

Line 79: one too many that

Fixed.

Line 145-146 vs lines 115-116: Repetition, please remove one of the two.

We modified the text near lines 145-146 to help address the Referees first comment, so there is no longer repetition.