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Interactive comment 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.

Anonymous Referee #2

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

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sion.

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?

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.

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.

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.

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 reader checking in Verney et al. (2011).

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.

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

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

Technical corrections:

Line 79: one too many that

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

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