Review of 'Downscaling sea-level rise effects on tides and sediment dynamics in tidal bays' by Jiang et al. submitted to Ocean Science

General comments

The paper by Jiang et al. presents a modelling study of a tidal bay, here the Eastern Scheldt, on the effects of sea-level rise on hydrodynamics and sediment transport. The study addresses an issue, which is of practical relevance to sediment management especially when considering future sea level rise.

Overall the paper is clearly structured and written in an easily comprehensible but precise way. The authors apply results from a European Shelf model to force a regional Eastern Scheldt model with different boundary conditions representing present-day and sea level rise (SLR) conditions. From these hydrodynamic modelling results they infer effects on sediment transport based on a simplified approach just taking the effect of M2 and M4 tides into account following Burchard et al. (2013). The authors conclude that the Eastern Scheldt will change with SLR from balanced flood-ebb current conditions to ebb dominance which will result in a loss of sediments of the Eastern Scheldt.

The results of Jiang et al. add an interesting case study to previous assessments of SLR effects on coastal regions. Therefore, their work is relevant to the scope of Ocean Science. The modelling study applies state of the art techniques of model coupling to combine large-scale changes of hydrodynamics due to SLR and regional dynamics of a tidal bay.

Based on their modelling results the authors draw the conclusion that the ES will change with SLR from balanced flood-ebb current conditions to ebb dominance and thus a potential loss of sediments. I find it hard to assess the model validity based on the presented results. I recommend a substantial improvement of model validation.

I appreciate the approach to infer sediment transport from hydrodynamic quantities; here the ratio of M2 and M4 tides, but this certainly requires to carefully checking if results are consistent and plausible. Here the authors have to spent considerable more effort.

In view of the complexity of hydrodynamic and sediment transport processes in tidal environments, the paper's very simplified approach to asses residual sediment transport lacks a critical discussion of results.

Scientific comments

a) Numerical modelling

- General

Title and wording in the article are misleading when it comes to the term 'downscaling'. The authors imply that the apply a new approach but teir methods are neither new nor would I call it downscaling per se. However, their approach is appropriate to address the scientific question how tidal dynamics of the Eastern Scheldt (ES) will change with SLR. The term 'downscaling' in the title is in my view misleading but scope and methods are clarified in the abstract.

- Hydrodynamics

One of the strong points of the paper is the analysis of possible resonance effects even though they do not prove to be important here. A shortcoming of the modelling work is the model validation. This holds for the general approach: not taking meteorological forcing into account is not well suited for a proper hydrodynamic model validation. This is especially important as the authors find that the presence of the storm surge barrier is relevant for SLR induced changes. Unfortunately the storm surge barrier is not resolved therefore they have to proof that they can cover the effect under sea-level rise conditions properly. Please carry out a comparison of model results and measurements outside of the ES to show that the model captures the transformation due to the storm surge barrier narrowing. Moreover, I would suggest carrying at least out a sensitivity study for high mean water levels in the North Sea but for an open storm surge barrier in order to assess if the model adequately resolves higher water levels in the ES.

Further issues concerning the representation of hydrodynamics are:

- Deviations of measurements and model results are rather large (up to 10 % in M4 amplitude), what about other error statistics (e.g. RMSE) and why would a further model calibration possibly not sensible?
- The effects of tidal flats are analysed in detail but how well are they represented by the model? You might want to give a comparison of hypsometric curves of model and underlying bathymetric data.

The discussion of the observed changes in tidal asymmetry in term of the interaction between the tidal wave and the basin geometry is going in the right direction, but is too short. You emphasize the importance of understanding the complicated interaction between basin geometry and tides. So why don't you give a thorough discussion on this aspect? For example, you indicate that tidal asymmetry depends on the a/h ratio as well as on the extension of intertidal area and find that ES becomes more ebb-dominant with SLR. So what does this mean with regard to the specific basin geometry of the ES and its significance for the tidal response to SLR?? Please consider further discussion on this aspect.

-Sediment transport

One of the main results of Jiang et al. is that the ES will suffer from sediment loss with SLR. Sediment transport is not modelled with a separate sediment transport model but inferred from hydrodynamics. This approach is on the one hand a simplification but is a neat way to obtain an estimate for important impacts of SLR. These results can have an important impact on local sediment management and the work of authorities in that area. Therefore, a careful model validation and sensitivity tests for sediment transport are required to prove reliable results. The validation needs to be generally improved.

It is important to understand the complexity of hydrodynamics and at least mention potentially relevant mechanisms at first before drawing conclusions on sediment transport. For example, tidal asymmetry is not only generated by M2-M4 phase differences, but also due to hypsometric controls and lateral circulation. Please state model limitations more clearly in the discussion.

Please consider the following issues:

- The assumption for sediment transport to be dependent on M2 and M4 amplitude and phase only is very crude as it neglects e.g. lateral circulation or density effects. Can you give any measurements for comparison?
- Are your results valid for the observed range of sediment grainsizes? At least carry out a sensitivity study on assumptions made (e.g. is settling velocity w_s of 1 mm/s representative for the ES).

- Why is the system experiencing sediment loss today (p. 6 l.13) if there is a mixed flood/ebb dominance?
- Results for Q are presented as averages over ES. What can we infer from that? I guess that is very difficult to interpret, e.g. you have a Q of about -0.35 kg/(m s) for an equal flood and ebb tide dominance (which would locally result in Q = 0). Please revise analysis or explain it.
- The authors estimate the net sediment transport Q inferred from the relation of Burchard et al. (2013). For a sea-level rise of 1 m Q would be around -0.8 kg/(m s). Please discuss the effect on the ES, e.g. taking the width of the ES at the mouth of about 10 km one would get an export of sediment of about 8,000 kg/s which relates with density rho 1,600 kg/m^3 to a volume loss of 158 Mio. m^3 p.a.. Is that sensible?
- A steady state topography is assumed for SLR which will not be the case, what kind of feedback mechanisms are to expect?

Even though the results needs further corroboration and discussion on model limitations I like to stress that the conclusions are presented in a clear and comprehensible way.

Specific and technical comments

- Abstract:
  - Clearly written but last sentence is misleading. One would expect way more than just model coupling.
  - 'our model downscaling approach' implies a novel approach for the model setup. However, downscaling is actually not new in modelling of shelf seas and shallow coastal waters.
- Introduction:
  - Their brief literature review is almost sufficient. However, some more reference to published work using their (coupled) modelling approach in other coastal regions would be appropriate. Are there no previous relevant studies for the ES itself?
  - Formally I would expect to find references in chronological order.
  - p.1 l.22 not only salt marsh accretion, but also tidal flat accretion is influenced (those two are not the same)
  - p.2 l.3 refer also to Pelling et al. (2013), who focused exactly on this aspect (the decrease in tidal amplitude as a consequence of enhanced dissipation in newly inundated areas)
  - p.2 l.5 Do you mean in shallow ebb-dominant estuaries?
  - p.2 l.5-8 A high ratio of a/h usually coincides with vast areas of intertidal flats, since large intertidal flats result in a small mean basin depth. Hence, there is NO contrast between the two aspects you describe as you say, too: seaward transport is reduced (first sentence) and there is a transition to flood-dominance (second sentence). This is the same direction of change in tidal asymmetry.
  - p.2 l.6 sentence mixed up
  - p.2 l.7 'may caus<u>e'</u>

- p.2 l.14 Is it really true that most modeling studies simply prescribe SLR as an additional water elevation at the seaward boundary? Please refer to some examples, where it was simply added.
- p.2 l.17 Pickering et al. (2017) use a global model and thus are probably no appropriate reference for statements on the tidal propagation on shelfs.
- p.2 l.23 as said in the general feedback: the downscaling method is already broadly applied for simulating tidal dynamics in coastal waters.

- Study site
  - p. 3 l. 4 water depths
- Methods
  - p.3 l.11-12 Add some more information about the model here. Are the models 2D or 3D? And if it is 3D, what is the vertical resolution? Which quantities are considered, which are relevant to density effects (transport of salt, heat)?
  - p.3 l.16 Better: 'The MARS domain extends to deep waters and covers the entire North-West European continental shelf...' This makes it more clear that the model not only extends to deep waters along a few sections of the open boundary, but captures the entire shelf edge.)
  - p. 3 l. 24 reference Slangen 2014 is missing; restructure sentence
  - p.4 l.10 Depending on the grain size, the time lag between local suspended sediment concentration and current velocity is not necessarily negligible. With regard to finer fractions (especially silt fractions) settling lag effects are important!
- Results
  - p.4 l.24 The correlation coefficient is not ideal to assess a tidal signal as the tidal wave itself is a very strong signal compared to the error, therefore observed and modeled tidal water elevations always have a relatively strong correlation - also in case of rather low model accuracy. For example, you may add RMSE, which is more appropriate.
  - p.5 l.8 "the main tidal patters" …of what? Tidal current velocity? Please clarify.
  - p.5 l.11-13 "With SLR, TR increases almost uniformly within ES": You may mention that this statement is related to the investigated SLR range up to 2 m and that this is not necessarily true for larger SLR values (e.g. for SLR > 2 m). (The same for p.5 l.19-21)
  - p.5 l.14 Please indicate more precisely, which region you mean with "adjacent North Sea"? Do you mean only the region directly located seaward from the barrier (up to which depth?) or the entire North Sea section within the GETM model or an even larger domain of the North Sea such as the Southern Bight?
  - p.5 l.16-18 I don't get your point. Why do you mention the fixation of the tidal basin size when talking about the role of tidal range for tidal prism? An increased tidal range will always increase the tidal prims, no matter, if the tidal basin is fixed or not. A non-fixation would only further increase the tidal prism. Also keep in mind that as long as intertidal flats are present in the initial case (your baseline scenario) the tidal prism will always increase with SLR, even if tidal range is not increased (remains constant), because with SLR former tidal flat volume is added to the tidal prism.

  - p.5 l.18 Figure 7 does not show that. It looks like tidal currents are mainly increased on the tidal flats or shoals. How could this be explained?
  - p.6 l.9-10 and Figure 6b: Your analysis is very difficult to justify. You made an average of residual sediment transport over the entire ES. What does that tell you about the exchange between ES and the adjacent North Sea?
    When the tidal asymmetry is 0, I would expect the residual sediment transport to be minimal as well, if it is related to the exchange between ES and the adjacent North Sea. Furthermore, I would expect the intensity of residual sediment transport at SLR of 0.25 m to be roughly the same as in the baseline scenario (SLR of 0), since the magnitude of tidal asymmetry of current velocity is about the same.
- Discussion and summary
  - p.6 l.17-18 Do you have any figure as proof of the value of 30 cm for the high water at spring tide? As shown in Figure 3 the increase in TR due to a SLR of 1 m is about 0.3 m. If tidal high water increases by an extra of 0.3 m (1.3 m in total), this means that tidal low water is elevated exactly by SLR (1 m in total)? So does that mean that the increase in tidal range by 0.3 m is solely induced by the increase of tidal high water?
  - p.6 l.19-23 “turnover time”: Why do you place this totally different aspect here? It is not logically connected to the rest of the text, neither to the preceding nor to the following.
  - p.6 l.24 stronger tidal response [of tidal range] to SLR
  - p. 7 l. 5 ‘increases faster than’ is unclear
  - p.7 l.6-8 I suggest not to generalize findings from this study, because the barrier is a special geometric feature strongly affecting the tidal dynamics in the ES (just as you say it in line 21).
  - p.7 l.11-15 A tidal basin with extensive intertidal flats actually corresponds to a high ratio of a/h. The relative importance of these two effects depends on the ratio of tidal flat area to channel area within the tidal basin. In your study site (ES) the channel area is much larger than the tidal flat area, suggesting a stronger dependence on the a/h ratio. This could explain why tidal asymmetry shifts towards ebb dominance with SLR.
  - p.7 l.27-28 I agree with you, but did you make any comparison to results of the shelf model (MARS)? If not, how can you conclude that shelf models are less applicable?
  - p.7 l.31 Are you sure that most studies on this topic neglected this aspect? I guess that most studies actually considered it.
  - p. 8 l. 16 you certainly take into account ‘gravitational force’, do you mean tide generating forces within the model

- Figures
  - The number of figures is adequate but the quality needs to be generally improved. A coastline and at least some geographic information would help.
  - Specifically: When you state Depth in m, what is the vertical reference system? Meter below NAP? IS it the same for regional and shelf model? What is the coor-

dinate system you are using? I would prefer coordinates instead of model dimensions for the axes.

- Figure 1: Cannot see gauge locations well.
- Figure 3: At least some geographic information would be nice, e.g. show Vlissingen from Fig. 1
- Figure 4:e) should be amplitude not phase in the
- Figure 6: references a) and b) missing
- Figure 7: show coastline / land