Interactive comment on “Downscaling sea-level rise effects on tides and sediment dynamics in tidal bays” by Long Jiang et al.
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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.

Response (1): Thanks for the constructive and detailed comments on our manuscript. We have revised the manuscript accordingly. Specifically, we improved the model validation by including additional statistical analysis and expanded the discussion as suggested. We have also interpreted the results on the sediment transport quantity *Q* with more caution given the limitations and simplifications of this method. Please see the following responses for detailed revisions.

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 their 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.

Response (2): We have changed "downscaling" to "model coupling" in the title and the text, where necessary. It is indeed not a new approach. Our main point is that it seems an appropriate solution compared to considering SLR only but not taking tidal changes on the shelf into account. It is clarified in Introduction (e.g, from Page 2 Line 34 to Page 3 Line 8 of the "accept-changes" version of the revised manuscript) and Discussion (e.g., Page 10 Lines 22–34 of the "accept-changes" version of the revised manuscript). Hereafter, wherever the page and line numbers occur, we refer to the "accept-changes" version.

- 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.

Response (3): Thanks for the comments. Please see the following four aspects addressing the comment.

1) About the missing meteorological forcing, the GETM setup for the Oosterschelde has been calibrated and validated with realistic meteorological forcing and gravitational circulation in our previous study (Jiang et al., 2019). In the current study, the key point is to simulate the "future"

SLR scenarios. It is difficult to include predicted meteorological forcing and river runoff for the future. Thus, both nested models MARS and GETM are run without meteorological forcing. Despite the absence of meteorological forcing in the model, the coupled model is able to simulate the water elevation with reliable performance, based on which scenario the further SLR runs were conducted.

2) The storm surge barrier is not unresolved. In Methods of the revised manuscript (from Page 4 Line 30 to Page 5 Line 2), we have clarified how it was implemented in our model. The storm surge barrier includes two manmade islands and three openings as shown in Fig. 1. Each tidal opening consists of concrete pillars and steel gates that can be closed under severes stormy conditions (Fig. R1). The bed of the barrier is secured by a sill resulting in a much decreased water depth (Fig. R2). The pillar is around 4 m wide, which is much narrower than the grid size of the model (300 m). The model would need a much finer resolution and smaller time step to resolve the pillars, which is computationally inefficient and unpractical. Given that the pillars accounts for only 8.2% of the overall cross-sectional area based on our calculation, we reduced the depth accordingly (i.e., by ~8.2%) to maintain the realistic cross-sectional area of each tidal opening of the barrier. It turns out that our model setting reasonably captures the realistic water elevation inside the Oosterschelde (Jiang et al., 2019).

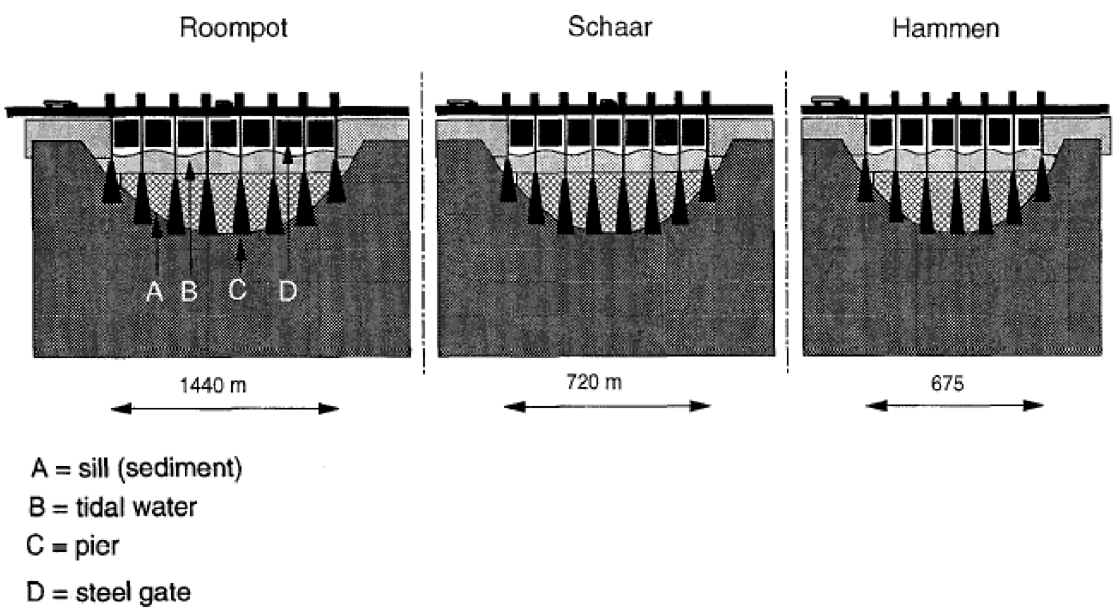

Figure R1: The schematic cross-sectional view of the storm surge barrier of the Oosterschelde. Source: Nienhuis and Smaal, 1994.

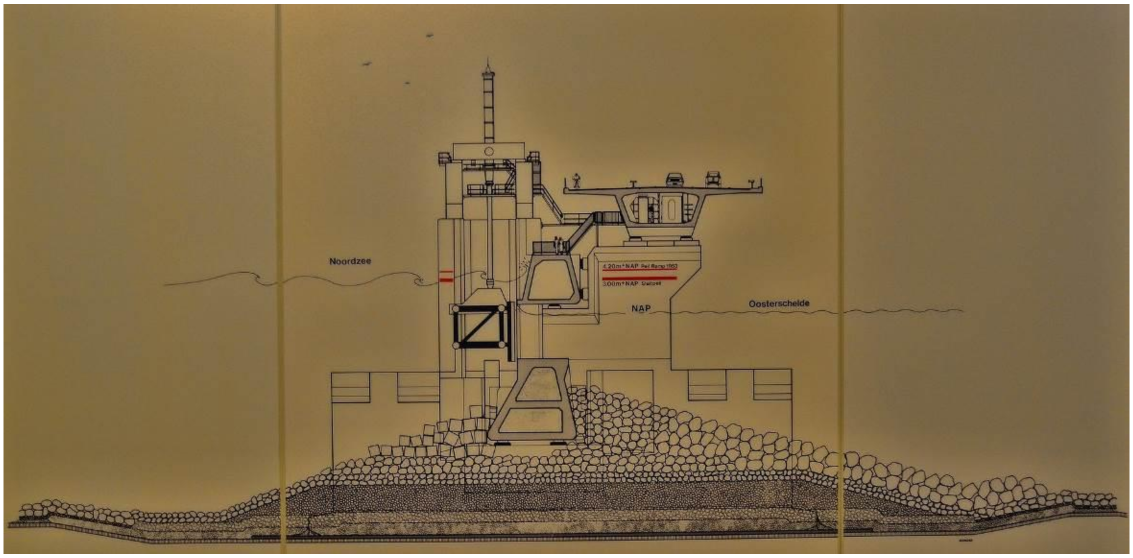

Figure R2: The side view of the storm surge barrier of the Oosterschelde. Source: Wikipedia Oosterscheldekering, https://en.wikipedia.org/wiki/Oosterscheldekering.

3) The calibration in the North Sea was conducted by Idier et al. (2017). To show the calibration results, we are here extracting the validation results (Figs. R3 and R4) from this paper, which indicate that the European Shelf model simulates the water elevation and major tidal components reasonably well.

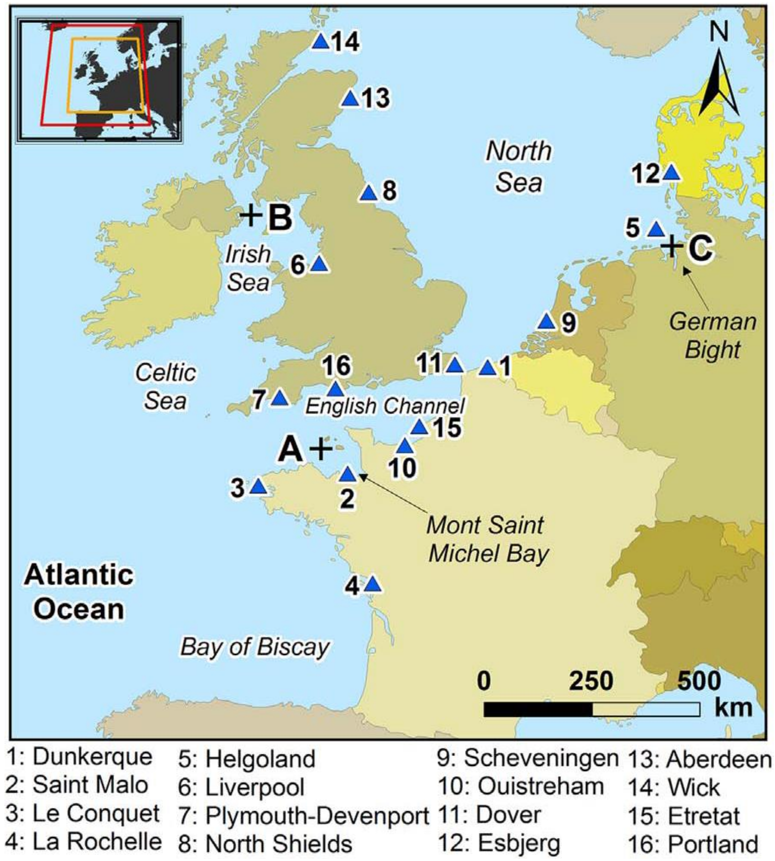

Figure R3: Computational domain of the MARS model with locations of tide gauges used to validate the model. Source: Idier et al., 2017.

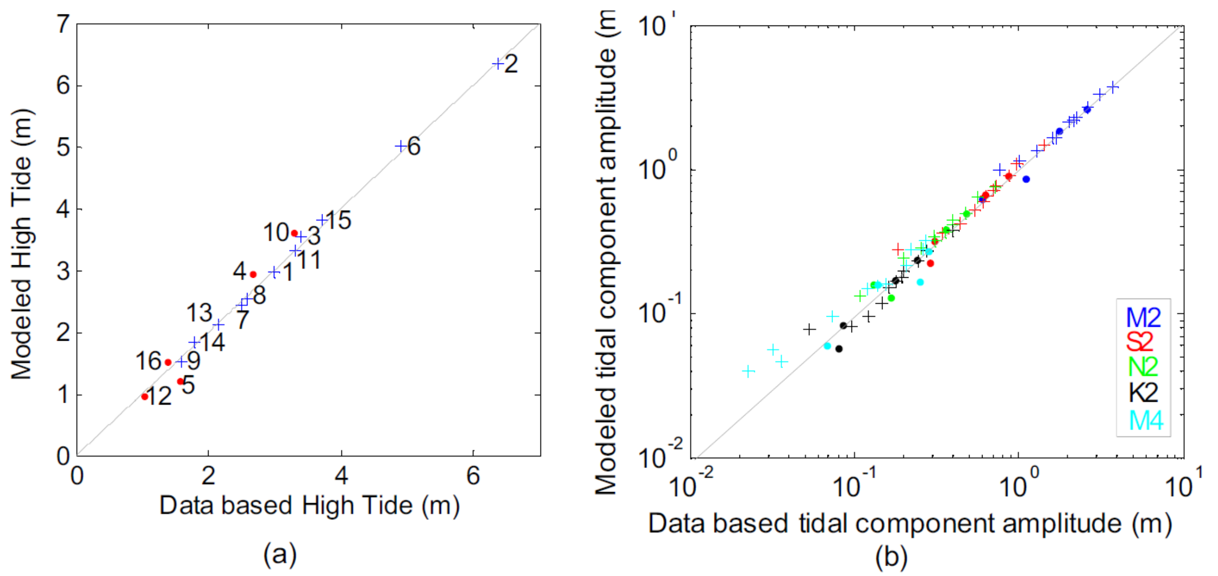

Figure R4: (a) Modeled versus data-based highest annual tide (for the year 2009) for each site. The numbers refer to the tide gauge names in Fig. R6; (b) modeled versus data-based tidal component amplitudes for 5 selected tidal components. + and • indicate sites with

relative errors for the highest tide that are smaller or larger than 5%, respectively. Source: Idier et al., 2017.

4) A sensitivity test without the storm surge barrier is a good suggestion but will not help the model validation. The current Oosterschelde is a completely different system from the pre-barrier (i.e., without the storm surge barrier) times not only because of the storm surge barrier. As part of the Delta Works in the late 1980s (https://en.wikipedia.org/wiki/Delta_Works), many other dams and sluices (Fig. R5) are built at approximately the same time cutting the freshwater input of the Oosterschelde, which along with other systems (e.g., the salt-water lake Grevelingen and freshwater lake Haringvliet) became isolated it from other delta networks (Ysebaert et al., 2016). Thus, running the pre-barrier scenario requires extending the model domain to the entire Southwest Dutch Delta including the Rhine, Meuse, and Schelde Rivers and may lend little weight to the model performance in the model setup of this study.

Figure R5: The Southwest Delta (Netherlands) with the main water basins and the main hydraulic infrastructures related to the Delta Works. Numbers indicate locations 1) Brienenoord, 2) Puttershoek, 3) Bovensluis, 4) Haringvliet center, 5) Steenbergen, 6) Zoom center, 7) Dreischor, 8) Zijpe, 9) Lodijkse Gat, 10) Hammen Oost, 11) Soelekerkepolder, 12) Hansweert. Source: Ysebaert et al., 2016.

References

Idier, D., Paris, F., Le Cozannet, G., Boulahya, F., and Dumas, F.: Sea-level rise impacts on the tides of the European Shelf, Cont. Shelf Res., 137, 56–71, https://doi.org/10.1016/j.csr.2017.01.007, 2017.

Jiang, L., Gerkema, T., Wijsman, J. W., and Soetaert, K.: Comparing physical and biological impacts on seston renewal in a tidal bay with extensive shellfish culture, J. Mar. Syst., 194, 102–110, https://doi.org/10.1016/j.jmarsys.2019.03.003, 2019.

Nienhuis, P. H., and Smaal, A. C.: The Oosterschelde estuary, a case-study of a changing ecosystem: an introduction, Hydrobiologia, 282/283, 1–14, https://doi.org/10.1007/BF00024620, 1994.

Ysebaert, T., van der Hoek, D. J., Wortelboer, R., Wijsman, J. W., Tangelder, M., and Nolte, A.: Management options for restoring estuarine dynamics and implications for ecosystems: A quantitative approach for the Southwest Delta in the Netherlands, Ocean Coast. Manage., 121, 33–48, https://doi.org/10.1016/j.ocecoaman.2015.11.005, 2016.

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?

- Response (4): We have calculated the RMSDs as shown in the Taylor Diagram in Fig. 2c. Why we do not do further calibration it that the GETM setup and settings are the same as in Jiang et al., 2019 except that the current study is a 2D barotropic run. That is, as discussed in Response (3), the only difference from the calibrated realistic run (Jiang et al., 2019) is the absence of meteorological forcing and gravitational circulation. When the wind forcing is not strong, the modeled water elevation is in good agreement with observation (Figs. 2a and 2b). Thus, most of the deviations from observation very likely originate from the missing winds and other realistic forcing, more than insufficient calibration. We have mentioned this in Section 4.1 of the revised manuscript.

  References

  Jiang, L., Gerkema, T., Wijsman, J. W., and Soetaert, K.: Comparing physical and biological impacts on seston renewal in a tidal bay with extensive shellfish culture, J. Mar. Syst., 194, 102–110, https://doi.org/10.1016/j.jmarsys.2019.03.003, 2019.

- 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.

- Response (5): Maybe this was confusing in our text. Our model does not account for the accretion and erosion of tidal flats. Rather a constant bottom topography is assumed in all scenarios. We have clarified it on Page 4 Lines 28–29. The bathymetry of tidal flats is derived from the observational data and does not change in the model run. They are the dataset measured by Rijkswaterstaat routinely and accessible online (http://opendap.deltares.nl/thredds/catalog/opendap/hydrografie/surveys/catalog.html).

- 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.

  Response (6): Thanks for the suggestion. We have emphasized the importance of basin geometry in modulating the tides in the Oosterchelde in the revised manuscript. Based on previous studies (Friedrichs and Aubrey, 1994; Hunt, 1964; Jay, 1991; Lanzoni and Seminara, 1998; Savenije and Veling, 2005; van Rijn, 2011), we have identified the basin as a strongly convergent and less strongly dissipative basin by the amplified wave speed and tidal amplitude, the tidal wave being nearly standing waves, and the landward decreasing basin cross-sectional area shown on the newly plotted Fig. 6. This part is detailed in Section 4.1 (Page 6 Lines 19–29). In the third paragraph of Discussion (Page 9 Line 32 to Page 10 Line 5), we explained the strengthened ebb dominance under SLR with the findings of Lanzoni and Seminara (1998). Under SLR conditions in our study, friction is reduced, while convergence of basin geometry does not change since the model does not allow flooding of shorelines. When convergence becomes stronger relative to friction, the basin becomes more distorted and ebb-dominant according to Lanzoni and Seminara (1998).

  References

  Friedrichs, C. T., and Aubrey, D. G.: Tidal propagation in strongly convergent channels, J. Geophys. Res. Oceans, 99, 3321–3336, https://doi.org/10.1029/93JC03219, 1994.

  Hunt J. N.: Tidal oscillations in estuaries. Geophys. J. R. Astron. Soc., 8, 440–455, https://doi.org/10.1111/j.1365-246X.1964.tb03863.x, 1964.

Jay, D. A.: Green's law revisited: Tidal long-wave propagation in channels with strong topography, J. Geophys. Res. Oceans, 96, 20585–20598, https://doi.org/10.1029/91JC01633, 1991.
Lanzoni, S., and Seminara, G.: On tide propagation in convergent estuaries. J. Geophys. Res. Oceans, 103, 30793–30812, https://doi.org/10.1029/1998JC900015, 1998.
Savenije, H. H. G., and E. J. M. Veling: Relation between tidal damping and wave celerity in estuaries, J. Geophys. Res., 110, C04007, https://doi.org/10.1029/2004JC002278, 2005.
van Rijn, L. C.: Analytical and numerical analysis of tides and salinities in estuaries; part I: tidal wave propagation in convergent estuaries, Ocean Dynam., 61, 1719–1741, https://doi.org/10.1007/s10236-011-0453-0, 2011.

-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.

Response (7): Thanks for the insightful comment. We should use caution when making the conclusion of the sediment transport of the entire basin under SLR. The revisions regarding to Q are mainly three-fold as follows.

Firstly, as the reviewer suggests, Q in our study is only one component of sediment transport that is affected by tidal asymmetry. Other components include lateral transport, density-driven transport and so on. Clarification has been made in Methods (from Page 5 Line 23 to Page 6 Line 5) and Results (Page 8 Lines 11–23) that our study only aims to look at how changes in tidal asymmetry impact sediment transport, not quantifying the sediment budget with all controlling processes included. The sum of Q over the entire basin that may be misunderstood as the sediment budget has been removed from Fig. 8b.

Secondly, the erosion parameter α and settling velocity w_s in calculating Q depend on the property (grain size) of sediments. When prescribing them, only one class of sand with a specific erosion parameter α and settling velocity w_s is considered. That is to say, our study aims to focus on the hydrodynamic effects (velocity and tidal asymmetry) that can be varied by SLR and affect sediment transport. The reason of choosing sand is that the Oosterschelde after the Delta Works is mostly sandy according to our along bottom measurements of sediment grain size distribution (Fig. R6) and a previous study (Fig. R7). That said, Q is only an example of how SLR can change the asymmetry-associated transport of this type of sand. We have specified it where Q is discussed in the revised manuscript. Please see from Page 5 Line 23 to Page 6 Line 5.

Thirdly, when calculating Q, we have changed from a constant eddy viscosity to the spatially variable eddy viscosity computed in the model. The results are updated in Fig. 7.

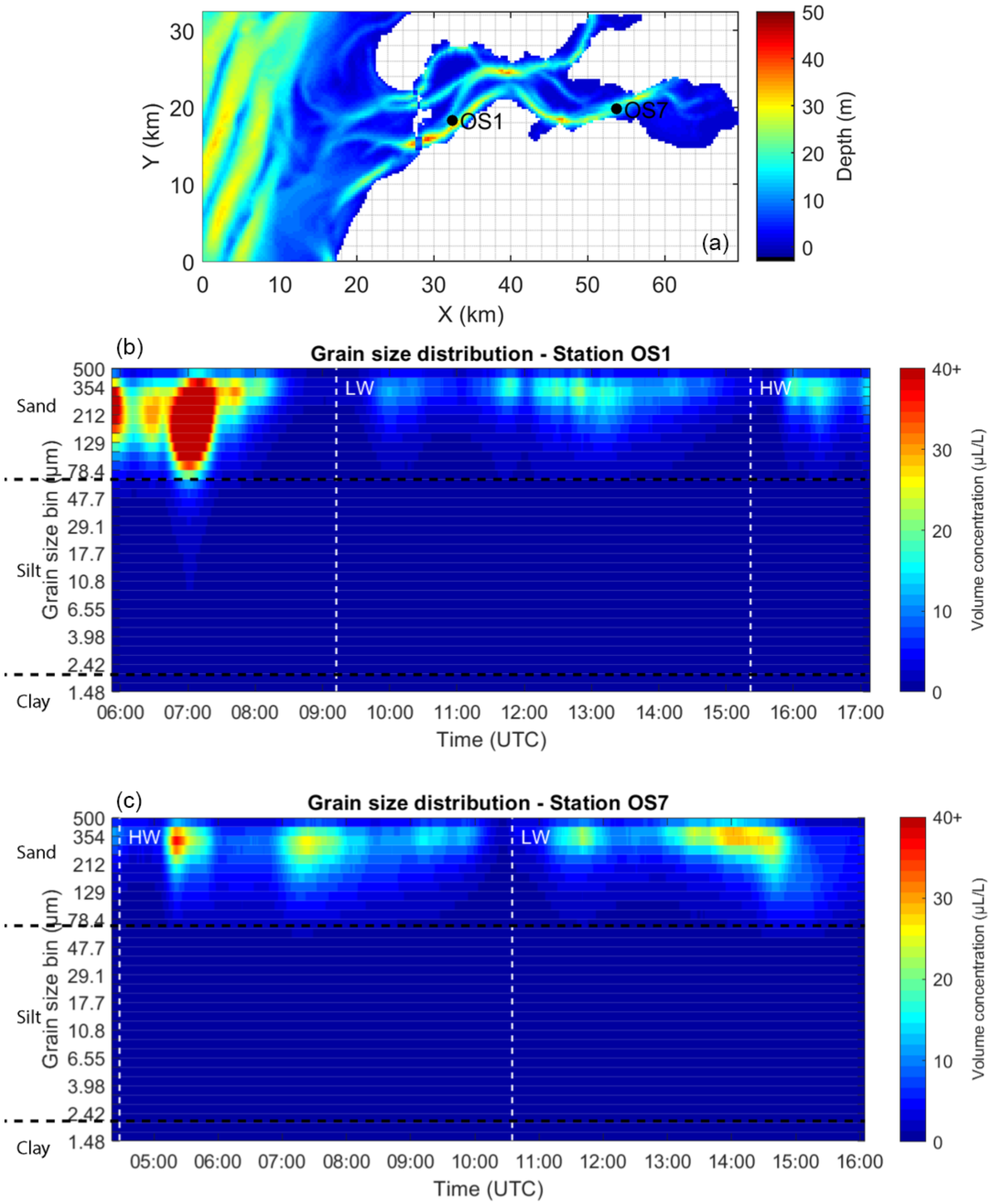

Figure R6: The near-bottom sediment grain size distribution at (a) two stations of the Oosterschelde: (b) OS1 measured on 4 June 2019 and (c) OS7 measured on 6 June 2019. The grain size distribution is measured by the LISST-200X Particle Size Analyzer.

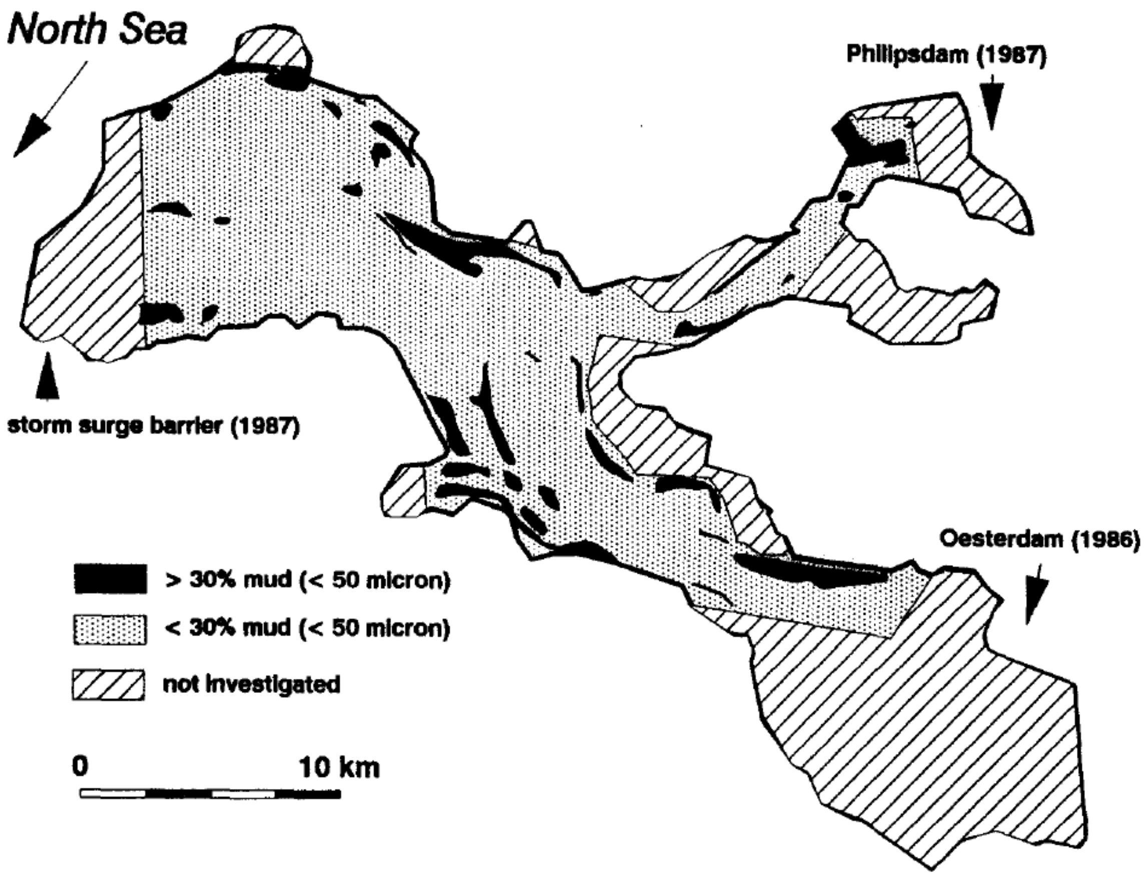

Figure R7: Fine sediment content < 53 pm of the subtidal bottom of the Oosterschelde after the completion of the storm-surge barrier and compartment dams. Source: Mulder and Louters, 1994.

References

Mulder, J. P., and Louters, T.: Fine sediments in the Oosterschelde tidal basin before and after partial closure, Hydrobiologia, 282/283, 41–56, https://doi.org/10.1007/BF00024620, 1994.

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?

  Response (8): We are not aware of studies comparing the effects of different processes on sediment transport in the Oosterschelde, which probably needs a sophisticated sediment model. Please see Response (7) for our revisions.

- Are your results valid for the observed range of sediment grain sizes? At least carry out a sensitivity study on assumptions made (e.g. is settling velocity ws of 1 mm/s representative for the ES)

  Response (9): Please see Response (7).

- Why is the system experiencing sediment loss today (p. 6 l.13) if there is a mixed flood/ebb dominance?

  Response (10): We have explained this in the paragraph in the revised manuscript. Please see Page 8 Lines 17–20. In the pre-barrier period, the sand erosion and sedimentation of tidal flats were in equilibrium in the Oosterschelde (Mulder and Louters, 1994). The storm surge barrier acts as a barrier of sand import and the reduced tidal currents cannot resuspend and supply sufficient sand to the eroded tidal flats, creating a sand deficit for tidal flats (Eelkema et al., 2012).

  References
  Mulder, J. P., and Louters, T.: Fine sediments in the Oosterschelde tidal basin before and after partial closure, Hydrobiologia, 282/283, 41–56, https://doi.org/10.1007/BF00024620, 1994.
  Eelkema, M., Wang, Z. B., and Stive, M. J.: Impact of back-barrier dams on the development of the ebb-tidal delta of the Eastern Scheldt, J. Coast. Res., 28, 1591–1605, https://doi.org/10.2112/JCOASTRES-D-11-00003.1, 2012.

- 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.

  Response (11): The quantity *Q* cannot be used as the sediment budget of the Oosterschelde given the drawbacks mentioned in Response (7). We have removed this from Fig. 8b.

- 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?

  Response (12): Please see Responses (11) and (7).

- A steady state topography is assumed for SLR which will not be the case, what kind of feedback mechanisms are to expect?

Response (13): A very good question. It is assumed that topography will not change with SLR. If it does, the spatial bottom roughness can be altered in contrast to the baseline scenario, which may change the local friction and tides. In addition, the convergence can also be changed. It will require a sediment transport and geomorphology model to study all these effects. An example is that the M2 tides in the German Bight are amplified because of the bathymetric changes (Hagen et al., 2019). We have expanded the discussion of the limitation in the last paragraph of the manuscript. Please see Page 11 Lines 18–24.

References

Hagen R., Freund J., and Plüß, A.: The impact of natural bathymetry changes, EGU poster, http://doi.org/10.13140/RG.2.2.13292.62083, 2019.

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.

Response (14): Thanks for the positive comment.

Specific and technical comments

- Abstract:
  - Clearly written but last sentence is misleading. One would expect way more than just model coupling.

    Response (15): We have change "downscaling" to "coupling" as mentioned in Response (2). Now on Page 1 Line 18.

  - '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.

    Response (16): Agreed. Our point is the model coupling approach should be applied more widely in such studies instead of simply rising the sea level of a regional model. This sentence is rephrased. Now on Page 1 Lines 18–19.

- 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?

Response (17): We have added references of coupled modeling approach in the last but one paragraph of Introduction in the revised manuscript. Please see Page 3 Lines 7–8. The previous literature on the Oosterschelde (we have changed the name “ES” to “Oosterschelde” according to another reviewer’s suggestion) is described in Section 2, Study site, specifically, Page 3 Lines 15–22.

- Formally I would expect to find references in chronological order.

  Response (18): The references throughout the manuscript have been changed from an alphabetical to chronological order.

- p.1 l.22 not only salt marsh accretion, but also tidal flat accretion is influenced (those two are not the same)

  Response (19): We have added tidal flat accretion to the sentence. Now on Page 1 Line 28.

- 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)

  Response (20): It has been added. Now on Page 2 Line 9.

- p.2 l.5 Do you mean in shallow ebb-dominant estuaries?

  Response (21): We doubled checked it. It should be flood-dominant here. Will explain it in the next response.

- 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.

  Response (22): Sorry for the confusion. We need more explanation here in describing the findings of Friedrichs and Aubrey (1988). Starting with one figure from this paper (Fig. R8b), when the ratio *a* / *h* is over 0.3, the M2-M4 velocity phase difference is over 270 degrees, which is an indication of flood dominance. The higher *a* / *h* is, the higher chance of flood dominance. If *a* / *h* is below 0.2, all estuaries tend to be ebb dominant with a M2-M4 velocity phase difference less than 270 degrees. When *a* / *h* is between 0.2 and 0.3, the tidal asymmetry depends largely on the ratio of tidal flats to channels *Vs* / *Vc*. Therefore, if *h* is increasing faster than *a* in a flood dominant system under SLR, i.e., *a* / *h* decreases, the system is moving to the left, i.e., towards less flood dominance. If *a* is increasing faster than *h*, the system become more flood dominant. With SLR, *Vs* / *Vc* will decrease, i.e., the system is moving down the y-axis, resulting in a more flood dominant situation. We have rephrased the sentence for clarification. Please see Page 2 Lines 11–16.

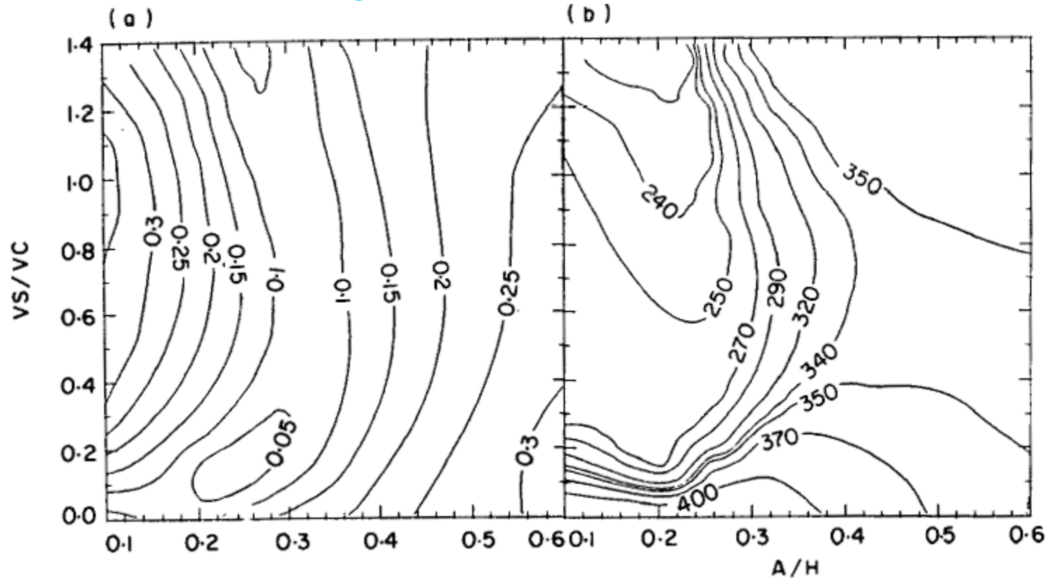

Figure R8: Contour plots of the numerical results of 84 model systems as a function of A/H and Vs/Vc: (a) cross-sectionally averaged velocity M4/M2, amplitude ratio; (b) cross-sectionally averaged velocity 2M2 – M4, relative phase. A, tidal amplitude; H, water depth; Vs, volume of intertidal storage; Vc, volume of channels. Source: Friedrichs and Aubrey, 1988.

References

Friedrichs, C. T., and Aubrey, D. G.: Non-linear tidal distortion in shallow well-mixed estuaries: a synthesis. Estuar. Coast. Shelf Sci., 27, 521–545, https://doi.org/10.1016/0272-7714(88)90082-0, 1988.

- p.2 l.6 sentence mixed up

  Response (23): "in" should be "if", but we have replaced the sentence. Please see Page 2 Lines 11–16.

- p.2 l.7 'may cause'

  Response (24): Sentence replaced.

- o 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 exam-ples, where it was simply added.

  Response (25): Examples provided. Please see Page 3 Line 2–3.

- 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.

  Response (26): Reference removed. Now on Page 3 Line 4.

- p.2 l.23 as said in the general feedback: the downscaling method is already broadly applied for simulating tidal dynamics in coastal waters.

  Response (27): The "broad applicability" has been changed to "necessity". Now on Page 3 Line 12.

- Study site
  - p. 3 l. 4 water depths

    Response (28): "depth" is changed to the plural form. Now on Page 4 Line 2.

- 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 con-sidered, which are relevant to density effects (transport of salt, heat)?

    Response (29): The model is 2D barotropic. It has been added to the Methods. Please see Page 4 Line 30.

  - 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.)

    Response (30): Thanks for the suggestion. The changes are made. Now on Page 4 Lines 14–16.

  - p. 3 l. 24 reference Slangen 2014 is missing; restructure sentence

    Response (31): The reference is added to the list on Page 17 Lines 26–28. We have rephrased the sentence. Now on Page 4 Lines 23–24.

  - 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!

Response (32): Thanks for the reminder. As mentioned in Response (7), we consider sand here. This part is revised to clarify the application of *Q* in this study. Please see from Page 5 Line 33 to Page 6 Line 4.

- 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.

    Response (33): Thanks for the suggestion. We have shown the Taylor Diagram including correlation coefficients, root mean square deviations, and standard deviations (Fig. 2c).

  - p.5 l.8 "the main tidal patters" …of what? Tidal current velocity? Please clarify.

    Response (34): We have removed this sentence in the revised manuscript.

  - 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)

    Response (35): Agreed. We have the defined the range in which these relationships applies. Please see Page 7 Lines 11–12.

  - p.5 l.14 Please indicate more precisely, which r egion you mean with "adjacent North Sea"? Do you mean only the region directly located seaward from the bar-rier (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?

    Response (36): Indeed unclear here. We have changed it to "the adjacent North Sea in the GETM domain as well as the Southern North Sea calculated by Idier et al. (2017)". Now on Page 7 Lines 17–18.

  - 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 base-line 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.

Response (37): Yes, it is true. This sentence is removed.

- 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?

  Response (38): Fig. 7 is now Fig. 9 in the revised manuscript. Fig. 9b shows the velocity difference between scenarios with 1 m and 0 m SLR. The difference is positive at most of the basin, indicating the increase of tidal currents in most areas. We explain it in Section 4.2. The M2 phase difference between the seaward to landward ends is reduced (Fig. 5d), which means that it takes shorter time for the M2 tide to penetrate along the basin. This is a combined result of increased water depth, reduced friction, and basin convergence. Please see from Page 7 Line 33 to Page 8 Line 3.

- 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.

  Response (39): We realize that the quantity Q in our study should not be used as the estimate of sediment budget in the Oosterschelde for reasons given in Response (7). Therefore, we have removed the basin-integrated Q in Fig. 8b, i.e., the previously Fig. 6b. Accordingly, the interpretation of Q is revised in Section 4.3. Please see Page 8 Lines 11–23.

- Discussion and summary

  - p.6 l.17-18 Do you have any figure as proof of the value of 30 cm for the high wa-ter 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?

    Response (40): The tidal range increased by 0.33 m for the 1 m SLR scenario, which is the average increase in tidal range. That is, the mean high water is increased and mean low water is decreased by half of 0.33 m, i.e., 0.17 m. Here, we are talking about the high water at spring tide, i.e., the maximum high water, which should be the upper limit of coastal defense. Fig. R9 shows the high water at spring tide in the baseline and 1 m SLR scenarios, and their difference. Excluding the 1 m SLR, the high water is elevated for an extra 20–30 cm (Fig. R9c). We have made the clarification in the text. Please see Page 8 Lines 27–30.

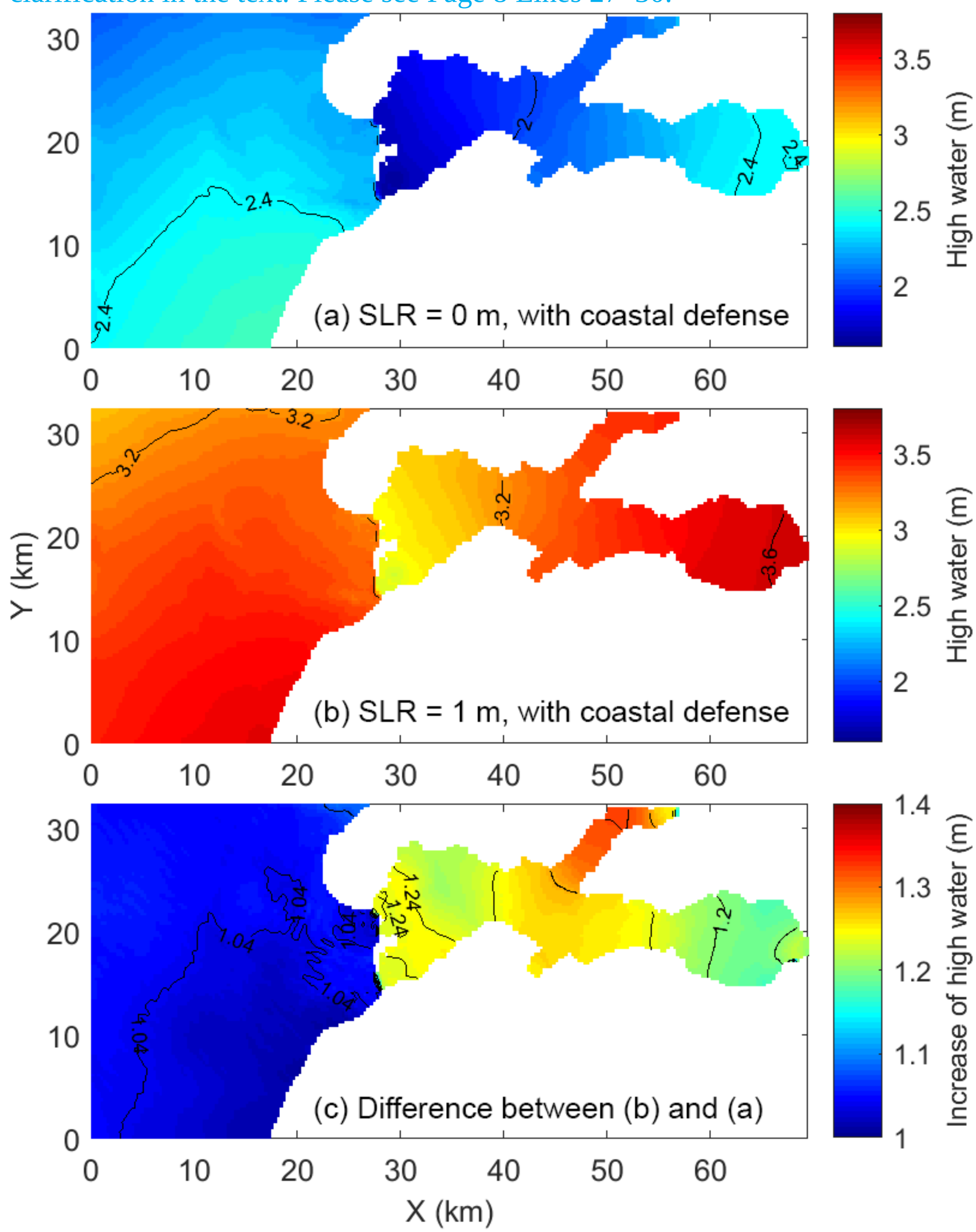

Figure R9: The high water during the spring tide in the baseline and (b) 1 m SLR scenarios; (c) the difference of high water between the 1 m SLR and baseline scenarios.

- 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.

  Response (41): Thanks for the comment. Maybe the text is not clear enough. The paragraph is about the impact of SLR on ecosystem functions and management strategies. Prior to this sentence, the impact of increased dike height is mentioned. The second impact is the potential changing ecosystem function. As mentioned in Section 2, the construction of the storm surge barrier almost doubled the flushing time of the basin and affected the exchange with the North Sea and ecosystem functions (e.g., shellfish culture) of the Oosterschelde. Since the tidal range may shift back to the pre-barrier level (Section 4.2), we are curious to see whether the flushing time can be reversed with SLR. This is why the sentence is placed in the paragraph. Now the paragraph is from Page 8 Line 25 to Page 9 Line 3.

- p.6 l.24 stronger tidal response [of tidal range] to SLR

  Response (42): The suggested revision is made. Now on Page 9 Line 4.

- p. 7 l. 5 ‘increases faster than’ is unclear

  Response (43): We have removed this sentence and updated the discussion in this paragraph (Page 9 Lines 4–24).

- 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).

  Response (44): Sentence removed.

- 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.

  Response (45): This makes sense. Like mentioned in Response (22), the ratio a / h is important. The ratio V_s / V_c in the Oosterschelde is small in the western part but is large in the east. That is probably the reason of spatial variable tidal asymmetry in the basin. We have expanded the discussion on shifts to ebb dominance in the Oosterschelde under SLR. Please see Page 9 Lines 26–32.

- 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?

  Response (46): The MARS model does not have a high resolution in the Dutch Delta region. The Oosterschelde is sometimes close (Fig. R10a) and sometimes open (Fig. R10b) to Grevelingen in MARS, while in reality they are isolated by dams and sluices. The water elevation in the Oosterschelde is also not well simulated due to a low spatial resolution. For example, the water elevation in the eastern part is always around 1 m whether at low (Fig. R10a) or high (Fig. R10b) tides. Therefore, a refined local model for the Oosterschelde seems necessary.

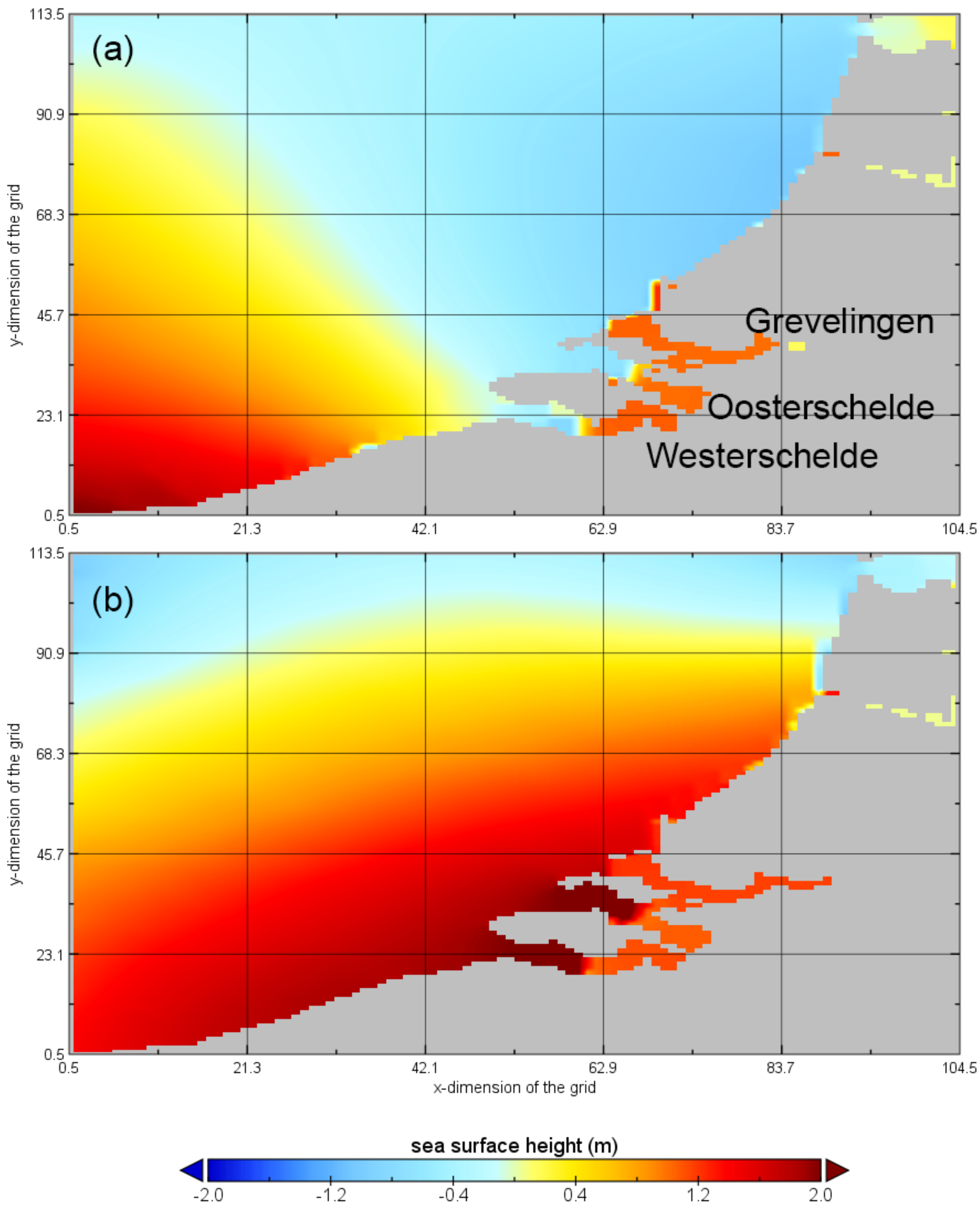

Figure R10: Snapshots of sea surface height simulated by MARS at (a) 2 Jan. 2009 14:15 and (b) 9 May 2009 14:00.

- p.7 l.31 Are you sure that most studies on this topic neglected this aspect? I guess that most studies actually considered it.

  Response (47): This is related to the comment addressed in Response (25). Our study emphasizes that it should be considered and not be neglected.

- p. 8 l. 16 you certainly take into account ‘gravitational force’, do you mean tide generating forces within the model

  Response (48): We have changed it to “gravitational circulation”. Now on Page 11 Line 15.

- 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.

    Response (49): We have changed the coordinates to longitude and latitude and added the coastline.

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

    Response (50): It is NAP. We used the Cartesian coordinate. Now we have changed them to longitude and latitude.

  - Figure 1: Cannot see gauge locations well.

    Response (51): Fig. 1 is replotted.

  - Figure 3: At least some geographic information would be nice, e.g. show Vlissingen from Fig. 1

    Response (52): Fig. 3 is replotted with longitude and latitude and Vlissingen is marked in the first panel. Now Fig. 4.

  - Figure 4:e) should be amplitude not phase in the

    Response (53): Fig. 4 is replotted and corrections made. Now Fig. 5.

  - Figure 6: references a) and b) missing

    Response (54): It is on the upper right corner. Now Fig. 8.

  - Figure 7: show coastline / land

    Response (55): We have added the coastline. Now Fig. 9.