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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In this manuscript, Jiang et al. describe a nested model in which a large regional model (2 km resolution) is downscaled to an estuary in The Netherlands (the Eastern Scheldt). Sea-level scenarios are run and it is shown that tide changes are much bigger in the estuary than in the North Sea. Moreover, increasing sea-level is observed to shift the estuary towards ebb-dominated currents, with implications for sediment transport. Overall, this is an interesting paper with some interesting results. However, the analysis and discussion of estuary tides and sediment could be improved, and many of the important papers and physical insights from the last decade or so could be referenced and used to help interpret the model results. The model is incompletely described, and more error statistics and discussion of sources of uncertainty would be good. In many places there are some additional analyses that could be done that would increase the novelty of the effort. Also, sediment transport in estuaries is complicated, and one usually should not ignore density/salinity effects; therefore, would suggest that the manuscript be more careful in how implications to sediment transport are described, and perhaps frame the discussion of results in terms of hydrodynamic quantities (e.g., relative phase) that strongly suggest that important components of transport have changed.

Response (1): We appreciate the reviewer’s constructive comments on our manuscript. The general comments include many specific ones that are addressed below. Specifically, 1) we have cited the papers suggested by the reviewer and included a deeper discussion of the convergent nature of the Oosterschelde and its responses to SLR. 2) The manuscript is extended to include details in model description and extra analyses (e.g., phase difference between horizontal and vertical tides, changes of M2 and M4 velocity, etc.) as suggested by the reviewer. 3) Caution is used to interpret the sediment transport quantity *Q*, which represents only one important component of sediment transport in the basin. *Q* has been recalculated using the eddy viscosity from the model. The parameters in the *Q* calculation are further substantiated.

Specific Comments:

Page 1

Line 18: “Global and regional tidal regimes”: While a few regional tide changes have been observed or modelled, ocean scale changes to tides have not been. Would suggest removing “global”.

Response (2): Thanks for the suggestion. Pickering et al. (2017) did indicate that SLR can induce changes in tides of the global ocean, despite that these changes may be much smaller than in shelf seas and estuaries. Thus, we tend to keep the "global" according to their findings. Now on Page 1 Line 24 of the "accept-changes" version of the revised manuscript. Hereafter, wherever the page and line numbers occur, we refer to the "accept-changes" version.

References

Pickering, M. D., Horsburgh, K. J., Blundell, J. R., Hirschi, J. M., Nicholls, R. J., Verlaan, M., and Wells, N. C.: The impact of future sea-level rise on the global tides, Cont. Shelf Res., 142, 50–68, https://doi.org/10.1016/j.csr.2017.02.004, 2017.

Line 23: The Chernetsky reference is 2010, not 2011.

Response (3): "2011" is changed to "2010". Now on Page 1 Line 28.

Line 25: "Nienhuis and Smaal, 1994": This is a rather old reference. Can you find a few others? There are a number of references about tidal changes and effects on currents, transport, salinity, sediment concentration, oxygen concentration, etc for estuaries such as the Ems, Gironde, Loire, Hudson, Western Scheldt, etc.

Response (4): Three references in the 2010s are added, Zhang et al., 2010; Winterwerp et al., 2013; de Jonge et al., 2014. Now on Page 1 Line 30 and Page 2 Line 1.

References

de Jonge, V. N., Schuttelaars, H. M., van Beusekom, J. E., Talke, S. A., and de Swart, H. E.: The influence of channel deepening on estuarine turbidity levels and dynamics, as exemplified by the Ems estuary, Estuar. Coast. Shelf Sci., 139, 46–59, https://doi.org/10.1016/j.ecss.2013.12.030, 2014.

Winterwerp, J. C., Wang, Z. B., van Braeckel, A., van Holland, G., and Kösters, F.: Man-induced regime shifts in small estuaries—II: a comparison of rivers, Ocean Dynam., 63, 1293–1306, https://doi.org/10.1007/s10236-013-0663-8, 2013.

Zhang, W., Ruan, X., Zheng, J., Zhu, Y., and Wu, H.: Long-term change in tidal dynamics and its cause in the Pearl River Delta, China, Geomorphology, 120, 209–223, https://doi.org/10.1016/j.geomorph.2010.03.031, 2010.

Page 2

"ramifications for residual sediment transport and morphodynamic development": Would suggest also referencing one of the more recent papers out of the Schuttelaars group (maybe the Dijkstra paper on the Western Scheldt) they have thought a lot about tidal asymmetry. Ton Hoitink probably also has some relevant papers, if memory serves.

Response (5): Suggested references (Hoitink et al., 2003; Dijkstra et al., 2019) are included. Now on Page 2 Line 11.

References

Dijkstra, Y. M., Schuttelaars, H. M., and Schramkowski, G. P.: Can the Scheldt River Estuary become hyperturbid?, Ocean Dynam., 69, 809–827, https://doi.org/10.1007/s10236-019-01277-z, 2019.

Hoitink, A. J. F., Hoekstra, P., and Van Maren, D. S.: Flow asymmetry associated with astronomical tides: Implications for the residual transport of sediment, J. Geophys. Res. Oceans, 108(C10), https://doi.org/10.1029/2002JC001539, 2003.

"Tidal changes due to SLR": would suggest also referencing the very nice Ensing et al. 2015 paper. There are many other papers on the effects of SLR on tides (another relevant one is Passieri et al., 2016). Somewhere, would suggest also referencing the forthcoming review papers on tide changes Haigh et al., 2019 submitted to Annual Reviews of Geophysics, and Talke & Jay, 2020, Annual Review of Marine Science (https://www.annualreviews.org/doi/pdf/10.1146/annurev-marine-010419-010727).

Response (6): Thanks for the four suggested interesting papers, which are added as references. The review paper in Annual Review of Marine Science is particularly relevant and helpful. Now on Page 2 Lines 18–30.

References

Ensing, E., de Swart, H. E., & Schuttelaars, H. M.: Sensitivity of tidal motion in well-mixed estuaries to cross-sectional shape, deepening, and sea level rise, Ocean Dynam., 65, 933–950, https://doi.org/10.1007/s10236-015-0844-8, 2015.

Haigh I. D., Pickering M. D., Green J. A. M., Arbic B. K., Arns A., et al.: The tides they are a changin', Rev. Geophys. In review.

Passeri, D. L., S. C. Hagen, N. G. Plant, M. V. Bilskie, S. C. Medeiros, and K. Alizad: Tidal hydrodynamics under future sea level rise and coastal morphology in the Northern Gulf of Mexico, Earths Future, 4, 159–176, https://doi.org/10.1002/2015EF000332, 2016.

Talke, S. A., and Jay, D. A.: Changing Tides: The Role of Natural and Anthropogenic Factors, Annu. Rev. Mar. Sci., 12, 14.1–14.31, https://doi.org/10.1146/annurev-marine-010419-010727, 2020.

"without considering tidal changes in the shelf seas that may propagate into estuaries/bays.": This is a good point. However, it is also true that tide changes in a bay or estuary could affect basin tides. see the Godin paper on Bay of Fundy, and the similar papers by Arbic, Garret, et al. Would suggest also including this detail here, and acknowledging in the Methods that feedback effects into the ocean are not modeled (or are they?) with your downscaling approach.

Response (7): We did not include the feedback from the Eastern Scheldt to the North Sea in the model downscaling, but we agree with the reviewer. It is necessary to mention this here in Introduction as well as in Methods. We have included the study on tides in Bay of Fundy and the Arbic and Garret paper in the Introduction (Page 3 Line 6). It is also added in the Methods "Our study applies a one-way nesting technique that accounts for the communication from the larger (MARS) to smaller (GETM) domain, but not the other way" (Page 4 Line 9–11).

References

Arbic, B. K., and Garrett, C.: A coupled oscillator model of shelf and ocean tides, Cont. Shelf Res., 30, 564–574, https://doi.org/10.1016/j.csr.2009.07.008, 2010.

Ray, R. D.: Secular changes of the M_2 tide in the Gulf of Maine, Cont. Shelf Res., 26, 422–427, https://doi.org/10.1016/j.csr.2005.12.005, 2006.

"tidal waves on the shelf are significantly modified in amplitude and phase": would replace "are" with "can be". When there is a steep shelf (e.g., US West Coast), there isn't very much modification that occurs.

Response (8): We agree with and appreciate the suggestion. Replacement made. Now on Page 3 Line 3.

Introduction, general comment: The introduction would be improved by surveying the local changes to tides that have been observed in the North Sea but also in the Western Scheldt, the Rotterdam waterway, etc. See for example Winterwerp et al. 2013, Cai et al. 2013, Hollebrandse 2005, or van Rijn et al 2018. There is an analogy to be made between channel deepening and sea-level rise, though the analogy is not exact. See again the Ensing et al. paper for dynamical insights. There are many changes on historical dredging effects that could be referenced.

Response (9): Two good points! The first suggestion is including overview of tidal changes in surrounding systems. We introduce the North Sea studies in Section 2, the Study Site, and have expanded to incorporate the suggested examples of the Western Scheldt and Rotterdam waterway. Please see Page 3 Lines 23–27. To address the second point, an extra paragraph is added to Introduction reviewing the tidal changes caused by channel deepening and discussing the implications on SLR-induced alterations of tidal properties. Please see Page 2 Lines 18–30.

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"are projected to increase mainly due to reduced friction": Isn't the changing amphidrome also a factor? Would suggest commenting on its relative importance.

Response (10): The movement of the amphidromic point is indeed important to the spatial variability of tides in the North Sea under SLR conditions. We have included the amphidrome migration here. Now on Page 3 Lines 29–30.

"tidal wave propagation can be Accelerated": not sure this is the best wording, since this would suggest constantly changing phase speed. Maybe "tidal phase speed is increased"?

Response (11): It is reworded as suggested. Now on Page 4 Line 2.

General comment: Use of acronym “ES” sometimes takes away from the understandability. you could consider just using the word Eastern Scheldt or Oosterschelde.

Response (12): “ES” is replaced with the Oosterschelde throughout the manuscript.

“MARS was forced”: could you comment in the text what guided the selection of these 14 constituents? Or in particular, why just one shallow water overtide? I presume M4 was quite small at the 200m isobaths, so is there any point in having it? Would be helpful to frame/ discuss some of these issues, to help clarify the modeling methodology.

Response (13): We took into account all the constituents available in FES2004.
For the M4 component has indeed a small amplitude along the open boundaries of the MARS domain. The amplitude is smaller than 1cm along the open boundaries, except close to the Norway coast where it reached few centimeters. Thus, we could expect our results to be same without forcing the MARS model with the M4 component.
To clarify the methodology, in the manuscript, we specific that we used all the available tidal components in FES2004.

Remove the “The” in “the prescribing both water”

Response (14): The redundant “The” is removed. Please see Page 5 Line 6.

General comment: Am glad you considered variable MSL forcing on the boundary. most studies do not do that.

Response (15): Thanks for the comments.

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“Every scenario was run for one year”: Can you comment on the consequences of missing the Sa and SSa constituents in your boundary forcing, which are probably larger than the fortnightly constituents you did include? Would be good to state what the magnitude of these constituents are, and what sorts of biases might be introduced by not including them. Or, stated differently, what do your sea-level rise scenarios suggest about seasonal variations in tide amplitudes, and at what point will sea-level rise effects be greater than seasonal variability?

Response (16): If we understand correctly, by boundary forcing the reviewer means the boundary of the MARS model. Since our model is forced by tides only, winds and gravitational circulation that can cause seasonal and interannual variability in tides are not modeled here. Thus, the Sa and SSa should not be a substantial component.

Sa is a radiational wave (it is generated by a cyclic geophysical phenomenon other than gravitational), neglected in our study, consistently with the fact that we neglect all atmospheric forcing. In terms of magnitude, the extraction from the FES2014 database (based on tidal modeling with altimeter and tide gauge data assimilation) at point (3.5°E, 51.75°N) provides the following amplitudes for Sa, Ssa, Msf, and Mf respectively: 0.0018 m, 0.005 m, 0.021 m, 0.0047 m. It suggests that in our study area, neglecting Sa has a negligible effect, while neglecting Ssa have an effect on water level probably smaller than 1 cm. These two components are smaller than the fortnightly constituents are. However, this is not a firm answer as FES2014 could also contains some errors.

"Vertical eddy viscosity Kv": why did you not use eddy viscosity from the model? Not sure using the same value everywhere makes sense. Also, is this a tidal average? At the very least it would be good to ascertain that your modeled eddy viscosity is consistent with this value. What I would guess is that velocity decreases quite a bit into the estuary (since velocity goes to zero at the head of tides), such that a constant eddy viscosity is a poor representation of reality. This is also a factor that will change with sea-level rise. Hence, might suggest looking into spatial patterns of eddy viscosity, and how they change with SLR. This is usually an easy output in a model, and would be something new (and would give insights into changed frictional effects).

Response (17): Thanks for the suggestion. Using the eddy viscosity from the model makes more sense in this case. Since our 2D barotropic run does not calculate eddy viscosity, we have changed the model run to a 3D run excluding winds and baroclinity. Note that the eddy viscosity is not a constant value but shows spatial variability (Fig. R1). The modeled eddy viscosity is on the order of 0.01 m^2/s, as is applied in our previous computation of Q. Despite that, we now plug the model output spatially-variable eddy viscosity in the calculation of Q and update the results in Fig. 7. The eddy viscosity does offer more insight into the hydrodynamic changes in the SLR scenarios. The viscosity over tidal flats are significantly enhanced compared to that over tidal channels. This may increase friction over tidal flats.

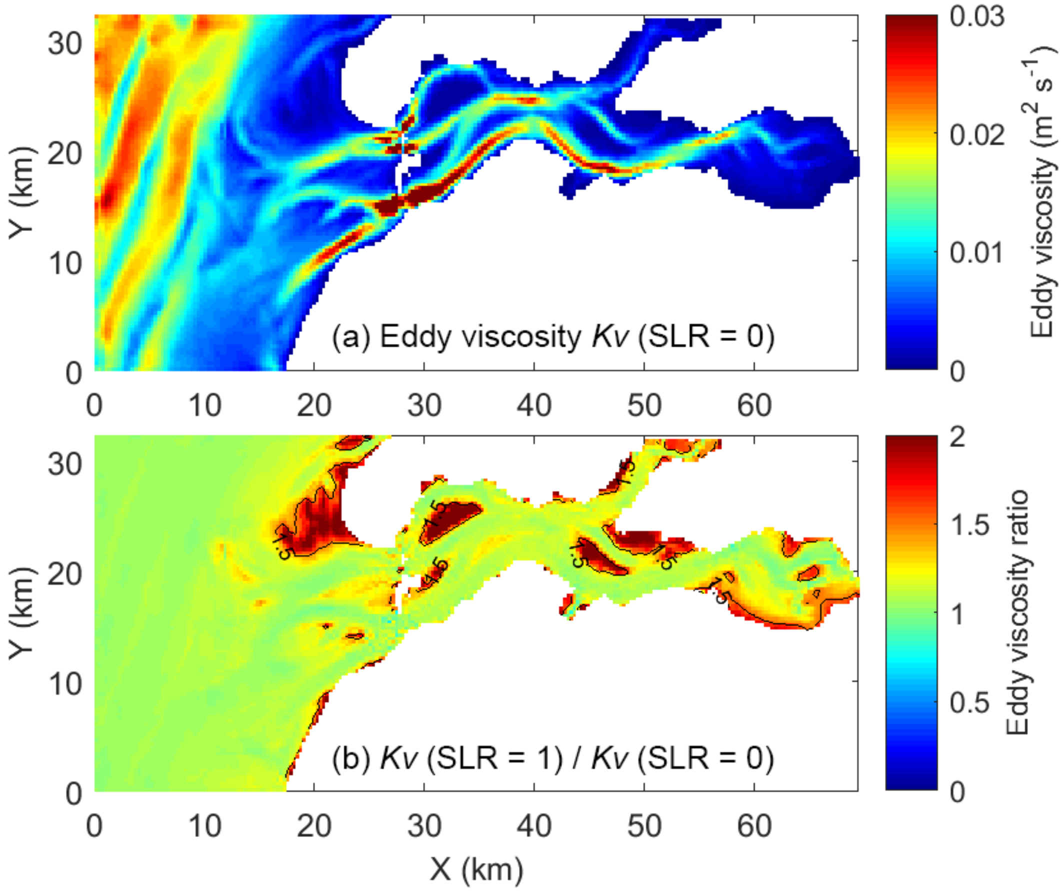

Figure R1: (a) Eddy viscosity in the baseline scenario and (b) the ratio of eddy viscosity between the 1 m SLR and baseline scenarios.

Constant erosion parameter. While this is used in Graewe et al 2014, is the assumption of a constant erosion parameter justified in an estuary in which sediment properties can be highly variable? Also, is this formulation valid for the cohesive sediments found in estuaries, which behave quite differently than sand? Finally, semi-analytical models in estuaries include both an erosion parameter (somewhat analogous to the one here) and an erodability parameter that is a strong function of location. This is because estuary turbidity maxima form within estuaries, changing sediment availability (i.e., some places have mud banks, others don't). Please look into and discuss more thoroughly the validity of the Graewe formulation within estuaries, and carefully frame what is not included here and what the consequences of that are. There is probably also specific information about the Eastern Scheldt that can be found in the grey literature or similar about sediment sizes, erodability, etc. that could/should be discussed and referenced to help place your results in context.

Response (18): It is true that the erosion parameter α and settling velocity w_s depend on sediment properties. The Oosterschelde sediment after the Delta Works is predominately sandy according to our near-bottom measurements of sediment grain size distribution (Fig. R2) and a previous study (Fig. R3). Therefore, we use a bulk erosion parameter and settling velocity for sand. However, although representative, our parameter cannot be used for all grain size classes. The text before revision reads like we are quantifying the sediment transport in these scenarios, which seems too aggressive given that Q is estimated in such a simplified manner.

The usage of Q needs to be clarified. We are not meant to map the current and future sediment transport here but imply that changing tides can alter sediment transport, or as suggested below, an important component of sediment transport. Therefore, we use the bedload transport of sand as an example of such potential changes and are more cautious about the Q description and interpretation. Please see Page 5 Line 23 to Page 6 Line 5.

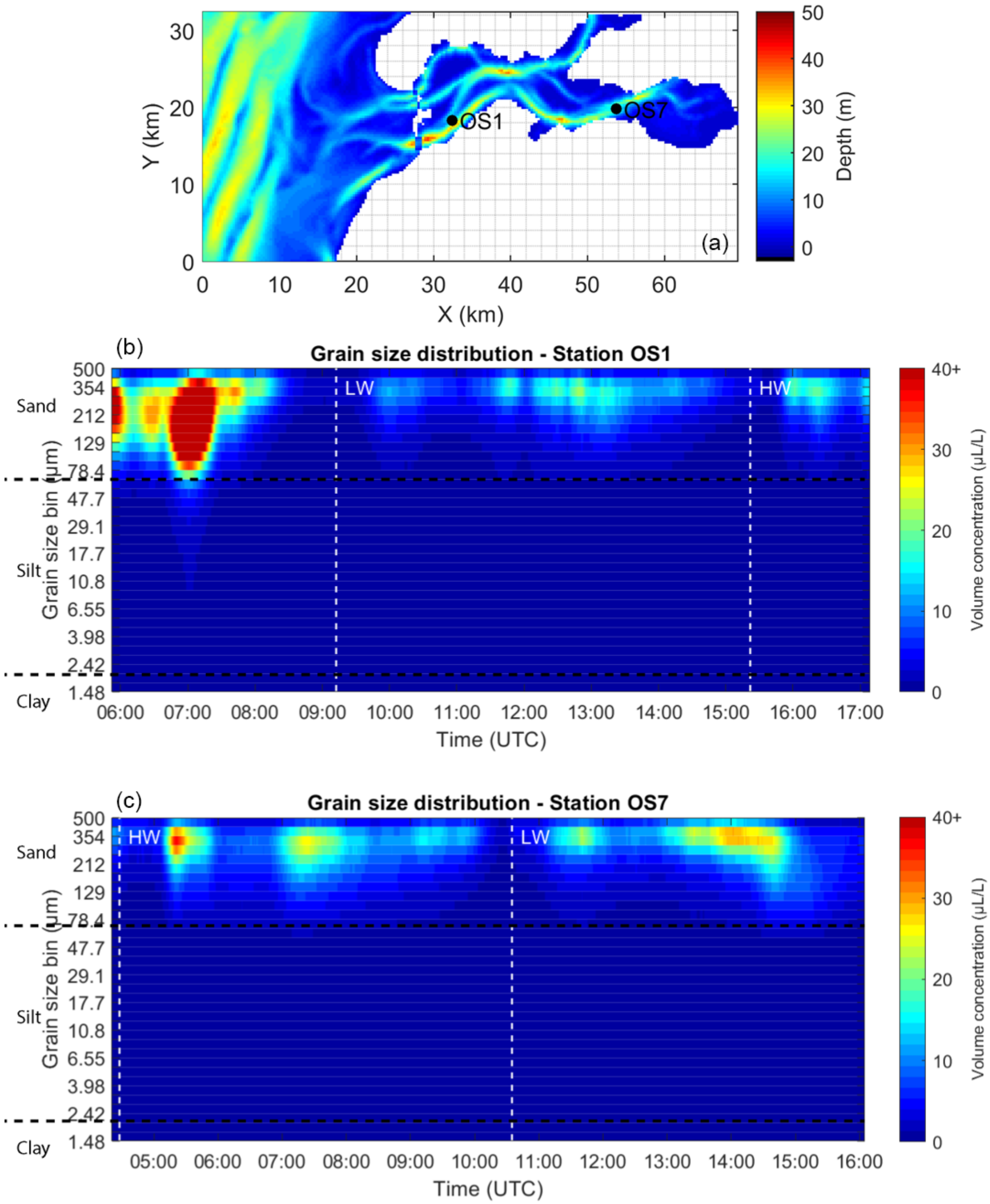

Fig. R2: The near-bottom sediment grain size distribution at (a) two stations of the Oosterschelde: (b) OS1 measured on 4 June 2019 and (c) OS7 measured on 6 June 2019. The grain size distribution is measured by the LISST-200X Particle Size Analyzer.

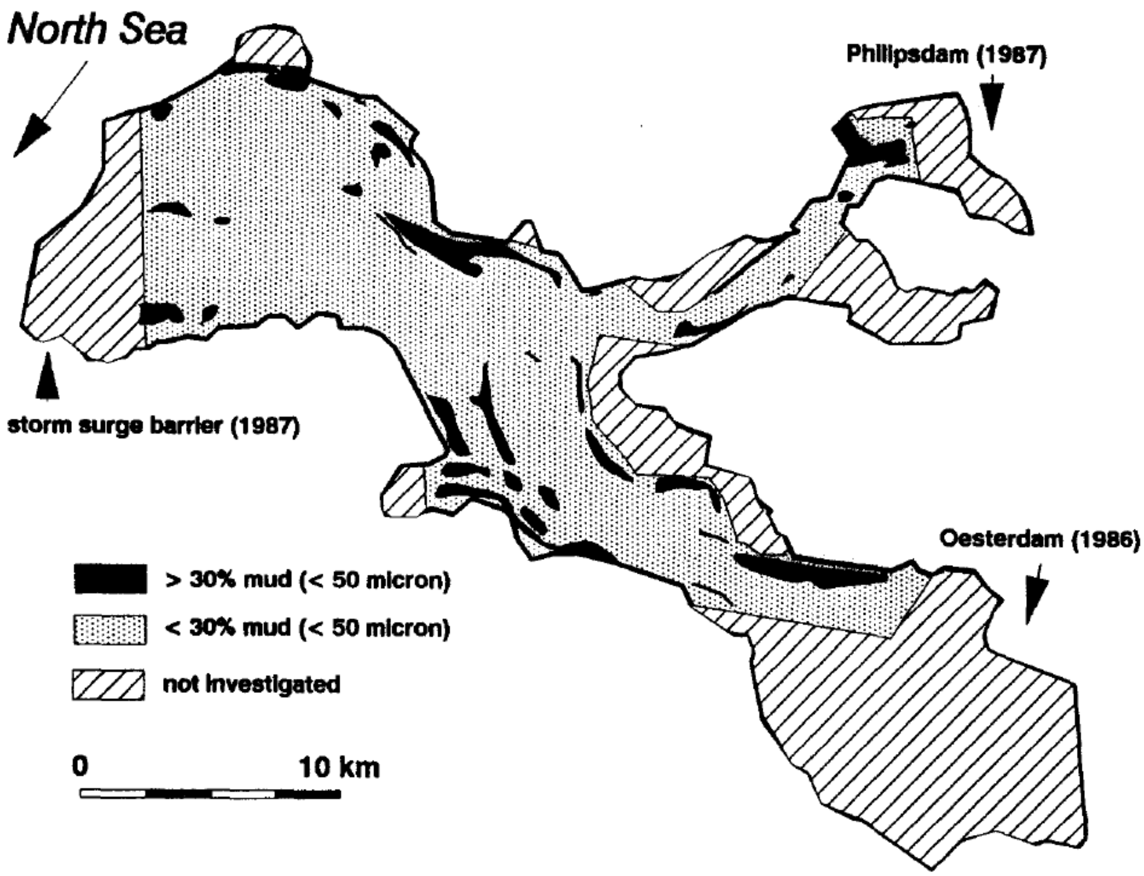

Fig. R3: Fine sediment content < 53 pm of the subtidal bottom of the Oosterschelde after the completion of the storm-surge barrier and compartment dams. Source: Mulder and Louters, 1994.

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.

General comment: there are other types of barotropic sediment transport that can be important besides tidal asymmetry (e.g., Tidal return flow, settling lag, etc). Would look at some the papers from the Schuttelaars group. Also, can you back up the assertion that gravitational circulation, internal asymmetry (now called "ESCO": see one of the Dijkstra papers) , and other types of tidal asymmetry are not important in the Eastern Scheldt, ideally with references or measurements? If there is a salinity gradient between ocean and freshwater, then it is at least somewhat important, in some places. There should be some information on this, and the salinity structure in the estuary should be discussed/referenced.

Response (19): Thanks for the suggestion. Yes, indeed these processes may have strong influences on the sediment budget in the Oosterschelde. With negligible freshwater input, the

Oosterschelde is more a tidal bay than an estuary. As mentioned above, the primary focus of the paper is more to seek the potential implications of tidal changes than to derive a complete picture of sediment transport in the basin. We have cited these processes in our revised manuscript. Please see Page 5 Lines 23–26.

"the directional changes in residual sediment transport in different SLR scenarios.": Given the various caveats mentioned above, would frame this as sensitivity of one component of sediment transport to SLR scenarios.

Response (20): A component of sediment transport is a good suggestion. We have rephrased in our revised manuscript.

General comment, methods: Did you account for the infrastructure at the Delta Works that caused tidal amplitude to decrease 13%, as stated earlier? Am not sure a resolution of 300m would be sufficient to model any bridge piers or storm surge structures in an adequate way. However, it is essential to model this infrastructure in some way: it would be incorrect to simply increase the drag coefficient in the entire estuary as a way to obtain realistic tides. In general, some description of the inlet infrastructure would be good (It looks like an Island was built, but there must be other structures as well).

Response (21): The storm surge barrier includes two manmade islands and three openings as shown in Fig. 1. Each tidal opening consist of concrete pillars and steel gates that can be closed under severely stormy conditions (Fig. R4). The bed of the barrier is secured by a sill resulting in a much decreased water depth (Fig. R5). 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). We did not change the drag coefficient in the bay for calibration purposes, and the same z_0 was applied as in the Wadden Sea (Duran-Matute et al., 2014). The description of the storm surge barrier and bottom roughness length scale is expanded in the Methods of the revised manuscript. Please see Page 4 Line 30 to Page 5 Line 2.

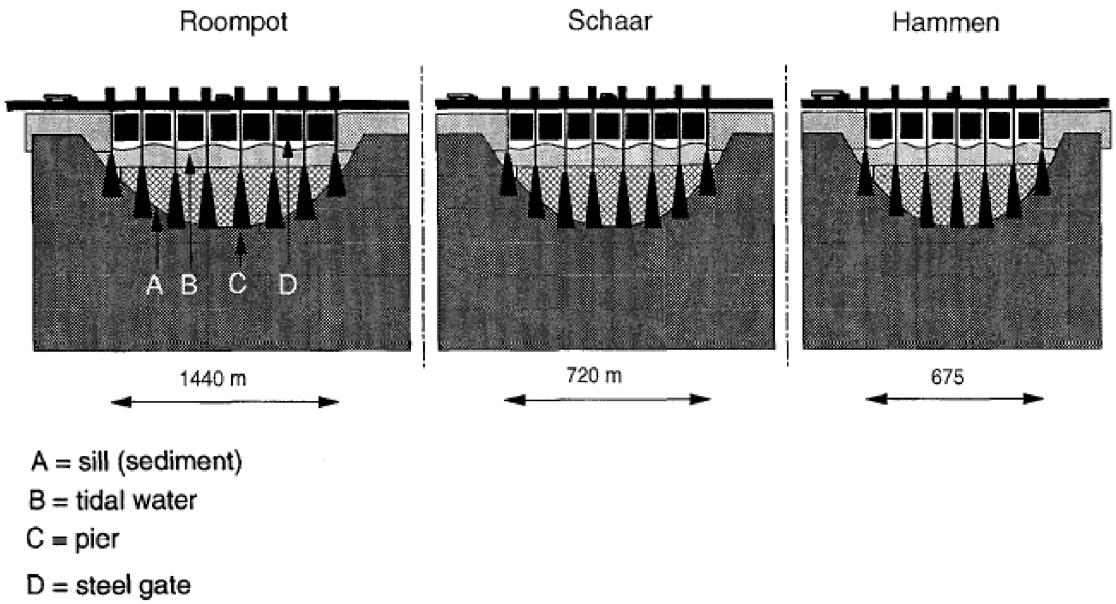

Figure R4: The schematic cross-sectional view of the storm surge barrier of the Oosterschelde. Source: Nienhuis and Smaal, 1994.

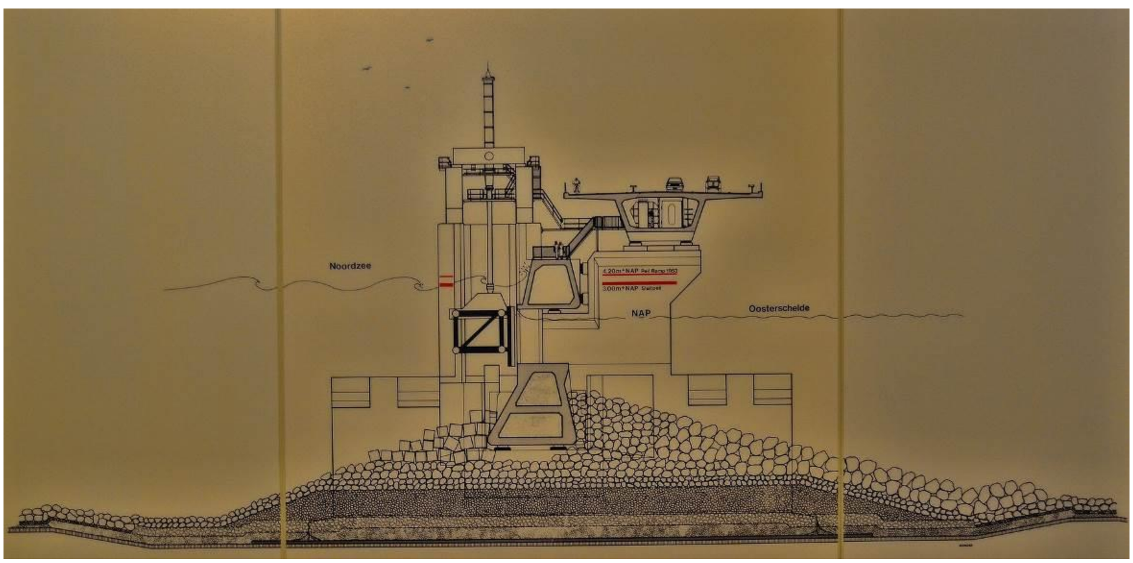

Figure R5: The side view of the storm surge barrier of the Oosterschelde. Source: Wikipedia Oosterscheldekering, https://en.wikipedia.org/wiki/Oosterscheldekering.

References

Duran-Matute, M., Gerkema, T., De Boer, G. J., Nauw, J. J., and Gräwe, U.: Residual circulation and freshwater transport in the Dutch Wadden Sea: a numerical modelling study, Ocean Sci., 10, 611–632, https://doi.org/10.5194/os-10-611-2014, 2014.

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.

A related note: Did not see any information about model calibration in section 3, even though section 3 promised (first paragraph) to discuss calibration. Information about tide stations used (and where to find data), statistics about root mean square error (for the different constituents), and so on is needed to assess how well the model is performing. Some of this information is given at the start of section 4, but it would be good to expand this.

Response (22): We have added the last but one paragraph to include the suggested information in Section 3 and the calibration results in Section 4.1. Please see Page 5 Lines 12–19 and Page 6 Lines 8–18.

General Comment: Was there wetting/drying in the model? This is very important for bathymetries in which there are intertidal flats, as there are here. For example, it can alter tidal amplitudes and tidal velocities. Please discuss whether you have wetting/drying, and the consequences if you do not (based off of known literature).

Response (23): Yes, the model resolves wetting/drying for tidal flats. We have added the description in Methods. Please see Page 5 Lines 4–5.

Section 4

Figure 2:Could you somewhere discuss the relative phase of the water levels (2M2 – M4) in your model, vs. the measured relative phase? This will give some indication about whether you are getting the tidal asymmetry correct.

Response (24): Good point. We have calculated the M2-M4 phase difference of the vertical tides and compared with the observations. Our model represents the direction of tidal asymmetry correctly despite overestimating the extent of flood and ebb dominance. We added a Fig. 3b for illustration. Please also see Page 6 Lines 13–18.

Also, it would be useful if your discussion of the calibration discerns between errors at the ocean boundary and errors that are produced within the estuary. In other words, can you discern between the “external M4” and the “internal M4”, as in Chernetsky et al. 2010? In that vein, in might be useful to extend your calibration and discussion to coastal gauges that are outside the estuary (e.g., Den Helder, Vlissingen, and some other nearby coastal gauges). Having only 3 calibration points is a rather small sample size, especially since the wider domain encompasses many tide gauges. It would be useful to know how well the larger model is doing (with comparison statistics).

Response (25): The validation of the MARS model is mostly done by Idier et al. (2017). We are here extracting the validation results (Figs. R6 and R7) from this paper, which indicate that the

European Shelf model simulates the water elevation and major tidal components reasonably well.

It would be difficult to completely decompose internally and externally generated errors. We made an effort to run a scenario in which tides from the 1-m SLR MARS model were prescribed to the open boundary but the sea level stays the same as the baseline. The M4 tide from this run was compared from the baseline run. We can see that M4 difference from the boundary hardly penetrates into the bay (Fig. R8). Therefore, it is likely that internal M4 dominates in the bay.

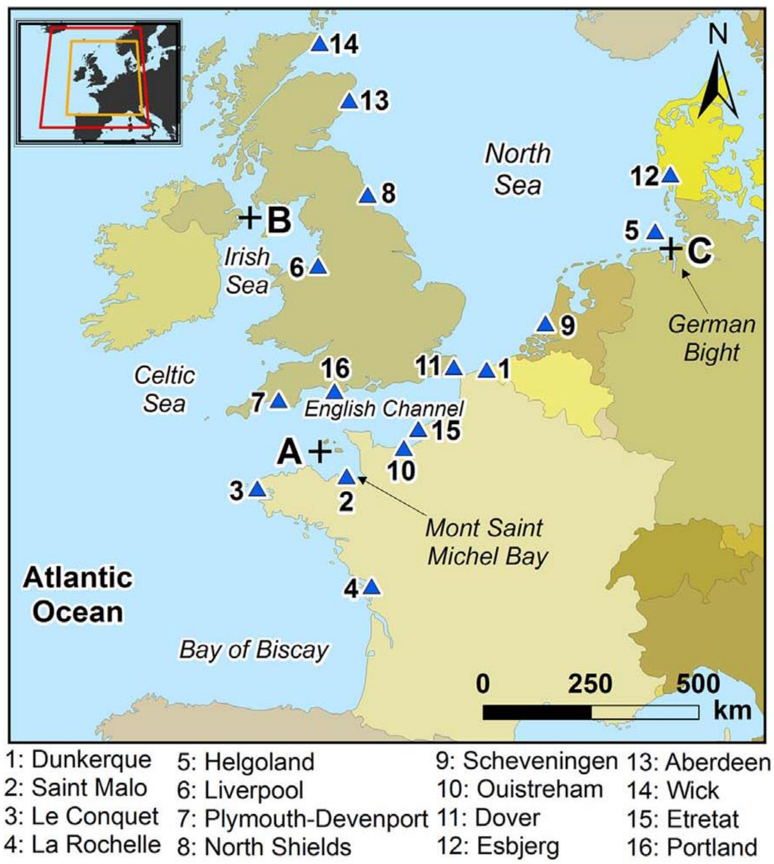

Figure R6: Computational domain of the MARS model with locations of tide gauges used to validate the model. Source: Idier et al., 2017.

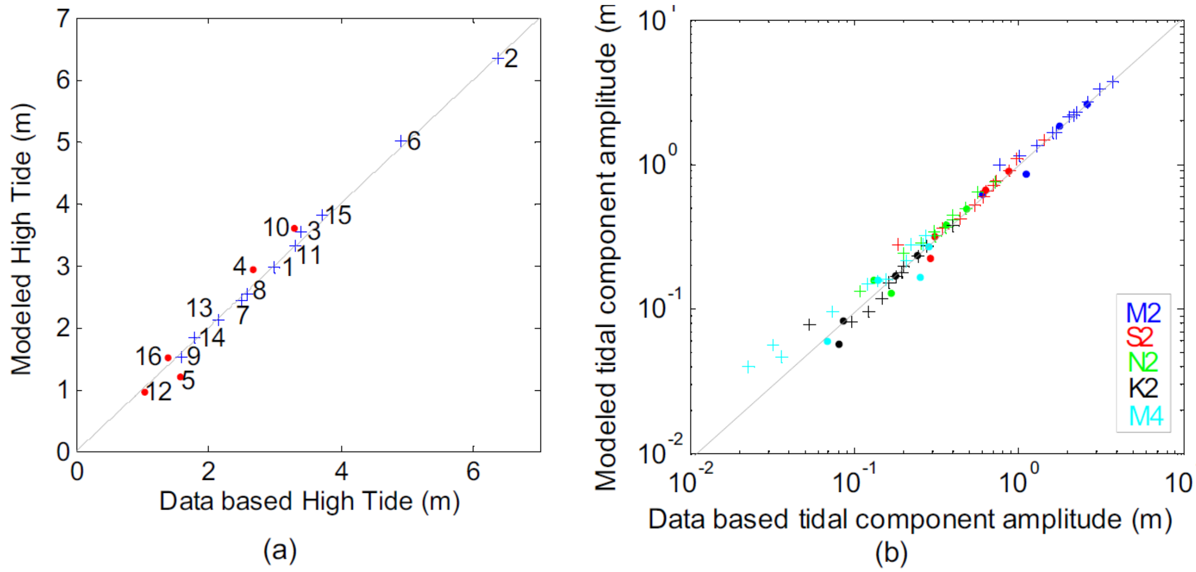

Figure R7: (a) Modeled versus data-based highest annual tide (for the year 2009) for each site. The numbers refer to the tide gauge names in Fig. R6; (b) modeled versus data-based tidal component amplitudes for 5 selected tidal components. + and • indicate sites with relative errors for the highest tide that are smaller or larger than 5%, respectively. Source: Idier et al., 2017.

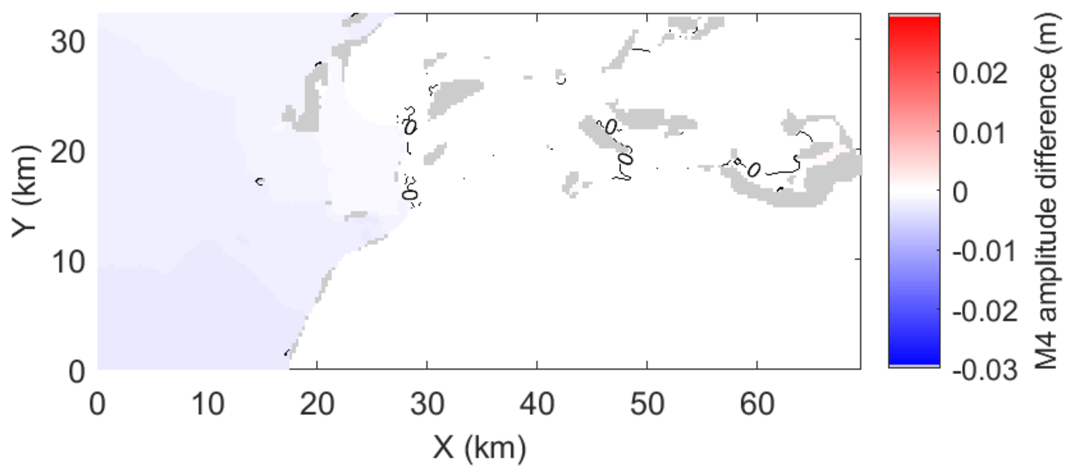

Figure R8: The difference in M4 amplitude in the GETM domain between a hypothetical model run and the baseline run. In the hypothetical run, the tides in the 1-m SLR scenario of the MARS model was prescribed to the GETM open boundary, while the sea level stayed the same as the baseline run.

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.

General Comment: Can you let us know what the phase between tidal velocity and tidal elevation is at different locations (e.g., for M2), and discuss implications? The phase provides insight into whether there is a Stokes Drift and an associated return flow (see e.g., Moftakhari et al. 2016).

Response (26): The phase difference between horizontal and vertical tides is close to 90º based on our calculation (Fig. R9). Thus, the tidal waves are mostly standing waves and Stokes Drift should be limited. The horizontal and vertical M2 phase difference helps us understand the tidal asymmetry calculated from both water elevation and tidal currents. We have included this in the last but one paragraph of Section 4.1. Please see Page 6 Lines 23–25.

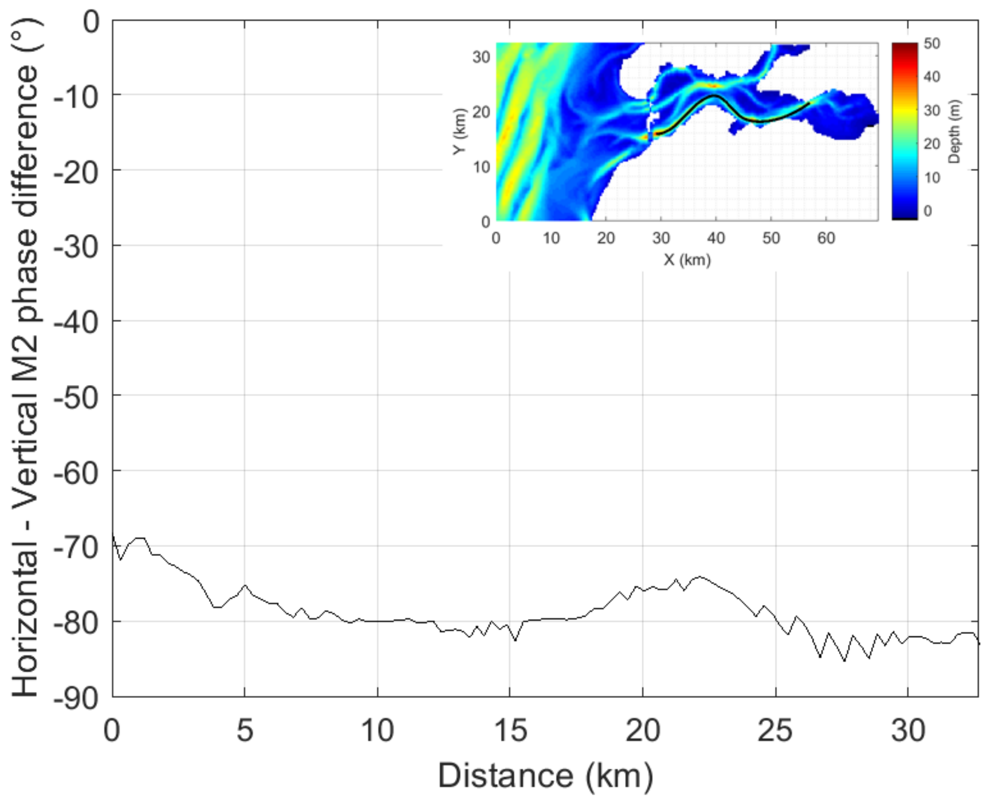

Figure R9: The M2 phase difference between horizontal and vertical tides (horizontal – vertical) on a transect from the west to the east of the Oosterschelde.

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These results are interesting. However, projecting into the future is fundamentally a counterfactual: it's a "what if" scenario that cannot (yet) be proven, yet depends a lot on the assumptions made in the future projection (flooding vs. no flooding, for example, or the assumption of no morphological change). Also, would argue that the modeled future tides depend a lot on how friction was modeled in the estuary, whether and how wetting/drying is included, etc. Further, small scale infrastructure (tide-gates) and small scale channels might (and probably do) matter. Some discussion of such uncertainties is needed. Again, the Ensing et al. paper has some insights.

Obviously one cannot include everything, and the comment above doesn't just pertain to this paper. However, can you think of ways to address what the consequences of various modeling decisions are, and discuss how they impact results? For example, how might trends with MSL change if friction is changed by +/- 10%? What would be the consequence of random perturbations in bathymetry, or if only the channels (but not the flats) get deeper (i.e, an assumption of partial morphodynamic adjustment)? Finally, might suggest running the model with and without the storm surge barrier infrastructure, to see if your model is able to approximate the historical change to the model. As argued in the Talke & Jay review and references therein, doing a retrospective model run is helpful in terms of making sure that your model can at least reproduce past trends (thus increasing confidence in future trends).

Response (27): We agree that predicting the future conditions are counterfactual relying on many assumptions, uncertainties, and unknowns. These uncertainties should be acknowledged and discussed in the manuscript. Our manuscript deals more with changes occurring by the "knowns", such as the regional SLR projected by Slangen et al. (2014) and the SLR-induced tidal changes in the shelf seas by Idier et al. (2017), rather than predicting the impacts of the "unknowns".

The concerns about the flooding and non-flooding setting is partially addressed in Response (23). The model setting is based on reality. The coastlines surrounding the Oosterschelde are protected by dikes where flooding is not allowed. The tidal flats can be flooded and exposed during tidal cycles. We discussed this in the last but one paragraph of Section 5 (Page 11 Lines 1–14).

In our study, a spatially constant bottom roughness length scale z_0 of 0.0017 m was used following Duran-Matute et al., 2014, which is added to Methods. How the bottom roughness will be affected by changing bed morphology in the future is highly unpredictable and thus we use the same z_0 in all SLR scenarios. This concern is recognized as one of the limitations of this study. Changing friction itself by ±10% seems unlikely because friction is nonlinearly related to turbulence and bottom roughness. To decipher how friction is influenced by bed forms and interacted with tidal currents, studies such as Cheng et al. (1999) and Prandle (2004) are necessary in the Oosterschelde. We have mentioned this limitation in the last paragraph of Discussion. Please see Page 11 Lines 18–21. The impacts of anthropogenic perturbation of the regional sea floor is also discussed in this paragraph referring to Ensing et al. (2015). Please see Page 11 Lines 24–27.

Running the pre-barrier simulation for additional model validation is a good suggestion. However, the current Oosterschelde is a completely different system from the pre-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. R10) were 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 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 this study.

Figure R10: 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

Cheng, R. T., Ling, C. H., Gartner, J. W., and Wang, P. F.: Estimates of bottom roughness length and bottom shear stress in South San Francisco Bay, California, J, Geophys. Res. Oceans, 104(C4), 7715–7728, https://doi.org/10.1029/1998JC900126, 1999.

Duran-Matute, M., Gerkema, T., De Boer, G. J., Nauw, J. J., and Gräwe, U.: Residual circulation and freshwater transport in the Dutch Wadden Sea: a numerical modelling study, Ocean Sci., 10, 611–632, https://doi.org/10.5194/os-10-611-2014, 2014.
Ensing, E., de Swart, H. E., & Schuttelaars, H. M.: Sensitivity of tidal motion in well-mixed estuaries to cross-sectional shape, deepening, and sea level rise, Ocean Dynam., 65, 933–950, https://doi.org/10.1007/s10236-015-0844-8, 2015.
Prandle, D.: How tides and river flows determine estuarine bathymetries, Prog. Oceanogr., 61, 1–26, https://doi.org/10.1016/j.pocean.2004.03.001, 2004.
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.

A similar comment is that at present the trends are given to 3 significant figures (e.g., 0.337m per m sea-level rise), which is almost certainly not justified when sources of error are considered. It will help the long-term “staying power” of the paper if the quoted figure could have some sort of confidence or certainty interval. The quoted error statistics on the line fit are not the same as the actual uncertainty: the close correspondence to a line shows that, within the assumptions of the model, there is a linear system response (which is interesting). However, the model results themselves are not perfect, as shown in the calibration (its perhaps the difference between precision and uncertainty).

Response (28): We would argue that the uncertainty level is at least 10%, since the RMSDs of the simulated and observed water elevation is 0.1 (Fig. 2c). This 10% deviation originates from the limitations of the model itself such as not including the wind forcing and gravitational circulation under our assumptions such as the consistent bed morphology. We have added these additional remarks to define the confidence level of the linear regression. Please see Page 7 Lines 12–15. In addition, we looked at the historical changing rate of tidal amplitude due to SLR. During 1900–1980, the tidal amplitude is amplified by 3%–4% in response to the 25 cm SLR (Vroom, 1994). This rate is comparable to our estimate, adding weight to our model results (Page 7 Lines 15–17).

References

Vroon, J.: Hydrodynamic characteristics of the Oosterschelde in recent decades, Hydrobiologia, 282/283, 17–27, https://doi.org/10.1007/978-94-011-1174-4_3, 1994.

“under SLR the M4 amplitude decreases outside, while it increases inside ES” – Please explain why.

Response (29): As shown in Fig. R8, the decrease in M4 amplitude in the North Sea is primarily a result of weakened M4 in the larger domain. The external M4 hardly penetrates into the bay. Therefore, the increased M4 amplitude in the basin is mainly a result of stronger tidal distortion.

“Tidal waves in shallow waters propagate at a speed of sqrt(gh)”: Actually, this is true only in the inviscid case (i.e., not your case). Would modify your text. Note that friction and convergence can strongly alter the phase speed. For your case, which is most likely weakly convergent and strongly (or moderately) frictional, would expect the phase speed to be somewhat less than sqrt(gh). Would suggest figuring out where in the parameter space mentioned above you are (e.g., by estimating your phase speed or by scaling), and discuss (the phase between velocity and water level also gives you an indication). In general, please look into the literature (e.g., Jay 1991, Friedrichs & Aubrey 1994, Lanzoni & Seminara 1998, and the many other idealized tide models) and discuss the processes in more detail, and how they affect results.

Response (30): Good suggestion that helps improve the manuscript. It turns out that the Oosterschelde is dominated by convergence more than friction. The main arguments include the greatly decreasing cross-sectional area landwards (Fig. 6), the increasing tidal amplitude landwards (Fig. 4a), a much faster M2 speed (22.7 m/s on average) than $(gh)^{0.5}$ = 8.3 m/s (Section 4.1), and the nearly standing wave of tides (Fig. R7). We have added these results in Section 4.1 (Page 6 Lines 19–29) and discussed our system as a convergent basin among references (Friedrichs and Aubrey, 1994; Hunt, 1964; Jay, 1991; Lanzoni and Seminara, 1998; Savenije and Veling, 2005; van Rijn, 2011). These analyses improve the interpretation of how the system evolves under SLR in Sections 4.2 (e.g., Page 7 Line 33 to Page 8 Line 3) and 5 (e.g., Page 9 Lines 19–21 and 32–33 and Page 10 Lines 1–5).

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.

“decline in bottom friction favors faster wave propagation”: without explanation, this doesn’t make sense. See comment above on frictional effects.

Response (31): The text here is revised according to the findings in Response (30). Now from Page 7 Line 32 to Page 8 Line 3.

General comment: To what extent is reflection of the tide wave important? Do you see evidence of resonance, e.g., in the phase plots (in near resonance you get a fast phase speed)? It would seem that in addition to changes in friction (and convergence) caused by depth changes, you may have changes in reflection or partial reflection. See e.g., Winterwerp et al., 2013, Familkhalili & Talke 2016, or Ralston et al., 2019. In reflective estuaries, the biggest change in tides is usually seen at the boundary; in estuaries where depth/friction changes matter most and reflection doesn't occur, the maximum tidal change is seen in mid-estuary (see again the Talke & Jay 2020 review). Some discussion on resonance is found later, I see, but some more close analysis is possible. One other idea would be to scale the relative importance of the convergence term and the friction term, to see if the rise in tide amplitude at the end of the estuary is due to friction that is weaker than convergence (e.g., Friedrichs & Aubrey, 1994).

Response (32): Thanks for the comment. Since the Oosterschelde is an amplifying basin, the phase speed is actually faster than the frictionless wave speed $(gh)^{0.5}$. The quarter-wavelength resonance period is 2 hours, even shorter than the calculated 5.5 hours. Therefore, it is further away from the M2 period. Thus, resonance is even less important than we first thought. The calculation is update in Section 5. Please see Page 9 Lines 7–16.

Page 6
General comment: Please explain why a transition to ebb dominance occurs. Perhaps the Friedrichs & Aubrey 1988 and Friedrichs & Madsen papers might have some insights.

Response (33): We have discussed the SLR-induced transition to ebb dominance in the paragraph from Page 9 Line 25 to Page 10 Line 13. The changes in tidal asymmetry caused by SLR depends on the competing effect of reduced friction versus submerged tidal flats (Friedrichs et al., 1990). The study by Lanzoni and Seminara (1998) offers insight into strongly convergent and less strongly dissipative basins such as the Oosterschelde. 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., Aubrey, D. G., and Speer, P. E.: Impacts of relative sea-level rise on evolution of shallow estuaries, in: Residual Currents and Long-term Transport, edited by: Cheng, R. T., Springer, New York, 105–122, https://doi.org/10.1007/978-1-4613-9061-9, 1990.

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.

"The quantity Q is used to estimate the combined effects of tidal current velocity and asymmetry". Before looking at Q, wouldn't it make sense to also plot out the M2 and M4 tidal currents (much like the amplitude plots)? It might also be interesting to see if the tidal ellipse change at all.

Response (34): We have plotted the tidal currents and its change with SLR (Fig. 9). Below we plotted the M2 and M4 velocity along the channel shown in Fig. R9. Only the major axis of the tidal ellipse is shown (Fig. R11) since the minor axis is several orders of magnitude smaller. SLR slightly increase the M2 and M4 velocity (Fig. R11), which is similar to the overall velocity in Fig. 9.

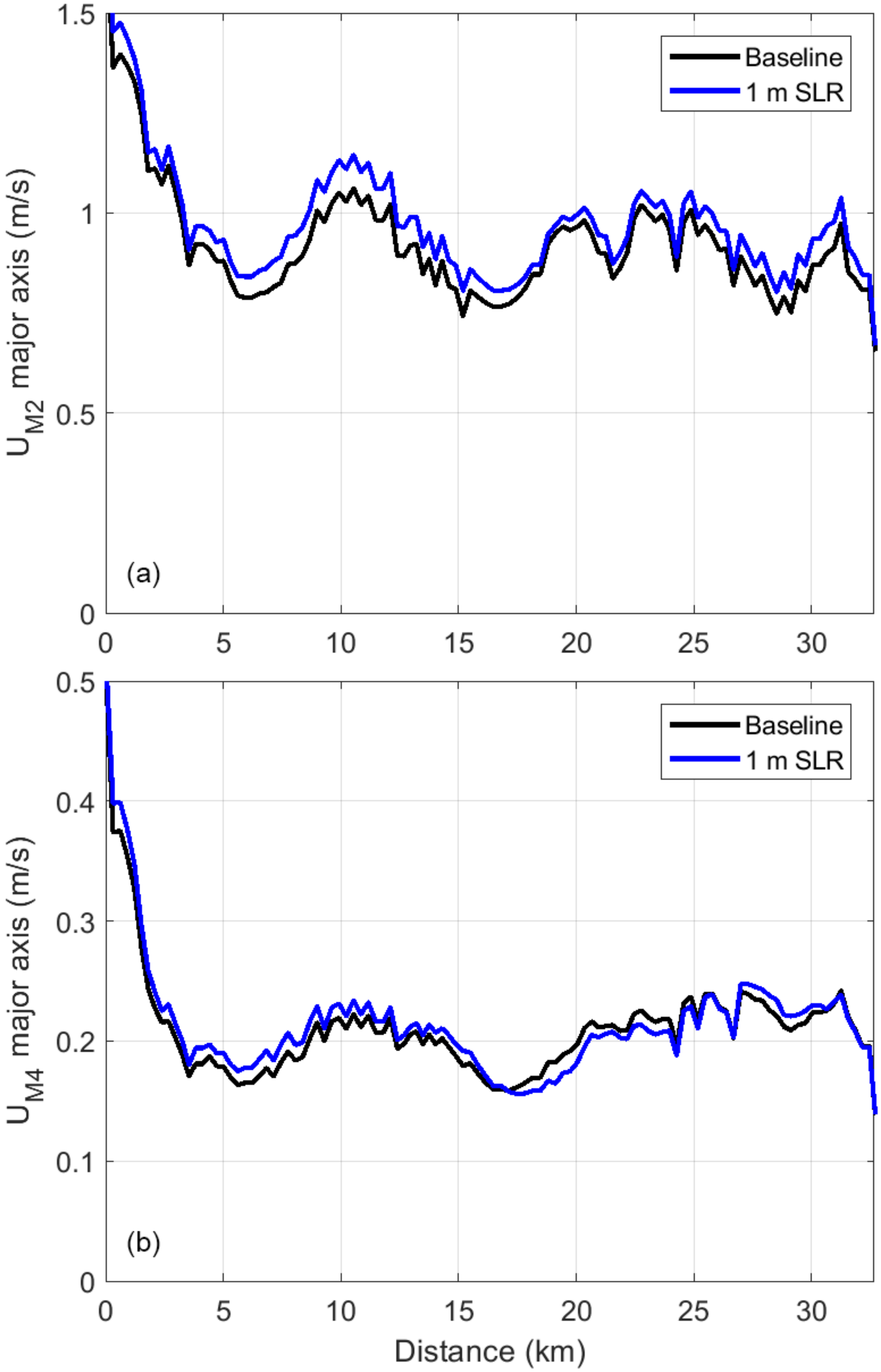

Figure R11: The major axis of the (a) M2 and (b) M4 tidal ellipses along the transect shown in Fig. R7 in the baseline and 1-m SLR scenarios.

“the residual transport more than doubles” Again, would be careful about calling “Q” the residual transport. It’s perhaps one type of residual barotropic transport, amongst many.

Response (35): We have removed this sentence and Q in Fig. 8b. Through the comments of reviewers, we realize that Q is only one component of tidal transport among many and only considers a typical class of sand. So, it was inappropriate to say Q is the overall residual transport. The purpose of calculating Q is to show that changes in tidal asymmetry may very likely alter the direction of sediment transport in the Oosterschelde. We have revised the entire manuscript to prevent the misleading thoughts that we quantify the overall sediment budget.

“this will not be accompanied by sufficient net sediment import as was in the past” Check grammar of this clause. Would also caution, again, about assuming that this is the only relevant source of transport. All coastal-plain estuaries that I’ve ever seen have a so-called estuary turbidity maximum that is caused by upstream transport. This is because baroclinic effects (ESCO, gravitational circulation) and settling lag effects are often so important. The paper would be helped by reviewing what is known about ETMs somewhere, both in general and in nearby estuaries (or ideally the Eastern Scheldt). The results presented here (and the way they are framed) would suggest that no ETM forms, which is probably not the case and would likely be greeted with skepticism in the ETM community. For references, see the Burchard et al. 2018 review and references therein.

Response (36): This sentence has been rephrased. We have also clarified that this is just one form of sediment transport. Please see Page 8 Lines 14–23. The Oosterschelde is more a well-mixed tidal bay than an estuary, because it receives negligible freshwater runoff after the Delta Works (Nienhuis and Smaal, 1994; Ysebaert et al., 2016). Gravitational circulation in the Oosterschelde is not as important as in a true estuary like the Western Scheldt. To our knowledge, no studies have found an ETM in the Oosterschelde.

References

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.

Discussion of resonance: Please give a general reference for Helmholtz resonance beyond the one given later in the paragraph from one of the co-authors (in any case, this is usually just relevant for harbors). Also, 5.3 hours is not that far away from the M4 frequency, for which you see a big (and unexplained) amplification. M4 resonance is not unheard of, and occurs for example in Hecate strait (Foreman et al. 1993). In any case, please dig deeper into your results and try to figure out whether you see any markers of resonance or altered reflection properties for any of your constituents (see also comment above). Regardless of the conclusion, this will improve your discussion.

A related comment: please discuss how you came up with the time scale of 5.3 hours. Did you use average depth and length? Or did you stress test your model with different frequencies and see what happens? The latter would give you a more accurate estimate. In any case, idealized models show that in frictional systems, the tide wave propagates slower than sqrt(gh), such that the resonant time scale is modified (increased). Moreover, resonance with friction is broad-band: there are a large range of frequencies that get amplified. (again, see Talke & Jay 2020 and references therein). Do such considerations impact your analysis? (would seem not for M2, but the point is that using an inviscid quarter wavelength is only an approximation and potentially misleading, and that the paper would be improved by thinking about this in a more sophisticated way).

Response (37): We have added another reference to the analysis of Helmholtz resonance (Sutherland et al, 2005) on Page 9 Line 13. The calculation of quarter-wavelength and Helmholtz resonance periods are based on the average length, width, and depth of the Oosterschelde. We have calculated the resonance period using the "corrected" phase speed, which is actually faster than $(gh)^{0.5}$. See Responses (30) and (32). The Talke and Jay (2020) paper discusses the response of resonance characteristics to changing depth (SLR). We have cited the finding in the discussion.

References

Sutherland, G., Garrett, C., and Foreman, M.: Tidal resonance in Juan de Fuca Strait and the Strait of Georgia. J. Phys. Oceanogr., 1279-1286, https://doi.org/10.1175/JPO2738.1, 2005.

Talke, S. A., and Jay, D. A.: Changing Tides: The Role of Natural and Anthropogenic Factors, Annu. Rev. Mar. Sci., 12, 14.1–14.31, https://doi.org/10.1146/annurev-marine-010419-010727, 2020.

Page 7

"frictional damping increases the semidiurnal tidal amplitude by 0.03-0.05 m/m SLR in the study region": Not clear what is meant by "study region". Please be specific.

Response (38): We have removed this sentence.

General comment about bathymetric effects: Agree these are important. Would suggest that you reference some of the studies that have showed similar effects of convergence, depth variation, etc. in the past (including but not limited to Ensing et al., 2015).

Response (39): We have discussed how tides in various embayments and estuaries (dissipative versus convergent, with choking effects, etc.) respond to SLR in this paragraph with examples and references. Please see Page 9 Lines 17–24.

"the Ems estuary may obtain a stronger flood-dominant signal": There were differences between the "external" M2 and "internal M2" in the Ems: basically, if memory serves, the decrease in damping (in part caused by fluid mud, in part by depth change) reduced the damping of the external M4 more than the "internal" or estuary M4 production was reduced. It would be helpful if you analyzed your results with this in mind… Also, what happens in the Western Scheldt? The Dijkstra et al. 2019 paper in ODYN discusses this estuary.

Response (40): The discussion of external and internal M4 is addressed in Response (25). The flood dominance in the Western Scheldt does not change much with deepening in the past decades (Winterwerp et al., 2013). We have expanded the discussion of SLR-induced changes in tidal asymmetry in this paragraph (from Page 9 Line 25 to Page 10 Line 13).

References

Winterwerp, J. C., Wang, Z. B., van Braeckel, A., van Holland, G., and Kösters, F.: Man-induced regime shifts in small estuaries—II: a comparison of rivers, Ocean Dynam., 63, 1293–1306, https://doi.org/10.1007/s10236-013-0663-8, 2013.

"Firstly, tidal responses to SLR can vary from system to system": I would say that this is already known. Perhaps modify conclusions, and make sure to include relevant references.

Response (41): This is not our major conclusion. These three paragraphs (firstly, secondly, and thirdly) discuss implications of our findings. We have added a "Conclusions" section to summarize the findings.

"and these effects may amplify in estuaries and bays.": Again, would point to the Arbic et al. 2009 and Arbic& Garret 2010 papers. There is also the potential for changed estuary tides to feedback into the basin. Any evidence of that?

Response (42): The insight has been added to the paragraph on Page 10 Lines 32–34. We did not account for the feedback from GETM to MARS, which will be interesting to investigate in the future.

"for instance in parts of the Chesapeake Bay". Did you mean SF Bay? There are some interesting papers for the Chesapeake that should be referenced. Lee et al., 2017 and Ross et al. 2017 (and Du et al. 2018).

Response (43): Yes, it should be San Francisco Bay. We have cited the Chesapeake Bay as well. Now on Page 11 Line 8.

“the gravitational force”: Not sure what you mean by this. Do you mean Gravitational circulation/baroclinic effects?

Response (44): It is changed to gravitational circulation. Now on Page 11 Line 15.

“Density-driven flow can also dominate local transport processes”: There are many other references, including reviews by Burchard et al. 2018 and Geyer and MacCready (2014) that address density circulation.

Response (45): These references are added. Now on Page 11 Lines 18.

Figure 1: The surge barrier should be labeled, not just shown with an ellipse. In general, it would be more helpful to describe exactly how much of the channel crosssection is impeded by the storm surge barrier, and how this is modeled.

Response (46): Thanks for the suggestions. We have added the description of the storm surge barrier and how it is modeled in Methods. See Response (21) for details.

Figure 2: Can you explain why only these specific days of tidal modeling are shown? Without explanation it could be interpreted as “cherry picking” a period of time where the fit was good. In general, more statistics on calibration would be good.

Response (47): The editor asked the same question before. We selected a period of weak wind forcing (Fig. R12), as explained in Section 4.1. We have also expanded the calibration with more statistics. See Section 4.1 and Fig. 2 for revisions.

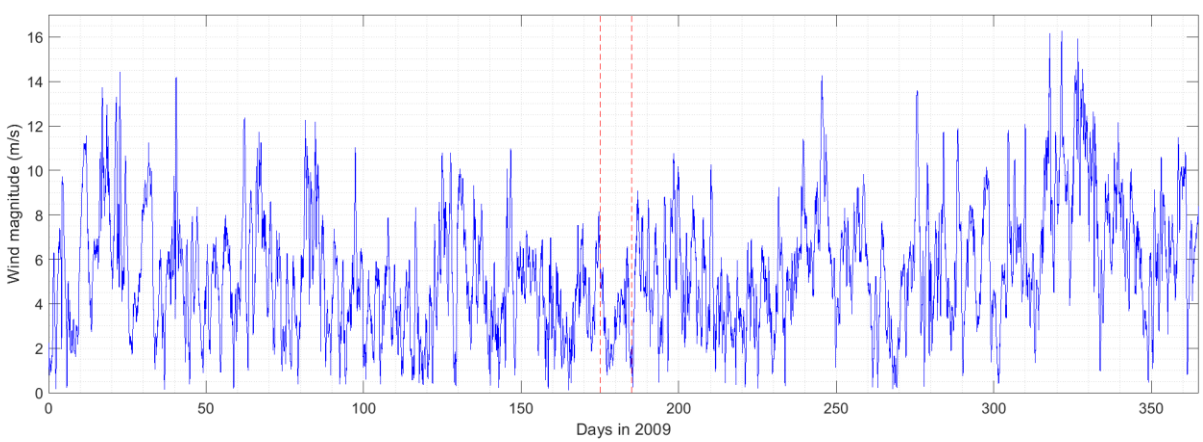

Figure R12: Wind magnitude over the Eastern Scheldt in the year 2009 (Data source: Royal Netherlands Meteorological Institute). The period of days 175-185 as shown in Figs. 2a and 2b is marked with read dashed lines.

Figure 3: How are you defining tidal range? There are different ways of doing that, so please specify.

Response (48): Tidal range is defined as the difference between high and low waters in every tidal cycle. Fig. 3 (now Fig. 4) shows the annual average of tidal ranges in all tidal cycles. We have added the definition in the caption.

Figure 4: The effect of the Delta Works is quite stark. Is there an effect of changing inlet cross-sectional area, i..e, as in Passieri et al. 2016? (That paper found variable changes to tides in back-barrier bays of the Gulf of Mexico. See also the Talke & Jay 2020 review for discussion on and references for the "inlet choking effect".

Response (49): Good point. However, in our study, the manmade islands and sill are not flooded by SLR, so it is not the case as in Passieri et al., 2016. We mentioned the choking effect on Page 9 Lines 21–24.

Figure 7: please provide information on how annual average was calculated. Is this based on peak velocity, rms velocity, average of the absolute value, or something else?

Response (50): This is root mean square current speed $(u^2 + v^2)^{0.5}$. We have specified it in the caption. Now Fig. 9.