

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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Review of “Downscaling sea-level rise effects on tides and sediment dynamics in tidal bays”, by Jiang et al.

In this manuscript, Jiang et al. describe a nested model in which a large regional model (2km 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, and will be a good contribution to the growing literature on sea-level effects on the hydrodynamics of es-

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tuaries. However, the analysis and discussion of estuary tides and sediment transport 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 more in terms of hydrodynamic quantities (e.g., relative phase) that strongly suggest that important components of transport have changed.

Specific Comments:

Page 1 Line 18 “Global and regional tidal regimes” While regional tide changes have been observed or modelled, ocean scale changes to tides have not been. Would suggest removing “global”

Line 23 The Chernetsky reference is 2010, not 2011

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.

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 may also have some relevant papers, if memory serves.

“Tidal changes due to SLR” Would suggest also referencing the very nice Ensing et

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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>)

“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.

“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.

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. In summary, there are many changes on historical dredging effects that could be referenced and provide validation that estuary tides can be very sensitive to bathymetry changes.

Page 3 “are projected to increase mainly due to reduced friction”. Isn’t the changing amphidrome also a factor? Would suggest commenting on its relative importance.

“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”?

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

“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.

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

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

Page 4 “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?

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? Please specify. At the very least it would be good to ascertain that your modeled eddy viscosity is consistent with this value. What I would guess (and your results show) 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

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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.

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.

“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.

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



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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).

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.

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).

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.

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, it 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).

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

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provides insight into whether there is a Stokes Drift and an associated return flow (see e.g., Moftakhi et al. 2016).

Page 5 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, unmodeled 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, but see also the Lee et al. 2017 paper for the Chesapeake.

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).

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 re-

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sponse (which is interesting). However, the model results themselves are not perfect, as shown in the calibration (the point I'm making is perhaps the difference between precision and uncertainty).

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

"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), though it's not too far from this value in many cases. 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.

"decline in bottom friction favors faster wave propagation"—without explanation, this doesn't make sense. See comment above on frictional effects.

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, Famikhali & 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 is not important, 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

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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).

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

“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 ellipses change at all.

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

“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.

Discussion of resonance: Please give a general reference for Helmholtz resonance

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beyond the one given later in the paragraph from one of the co-authors (in any case, Helmholtz resonance 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 $\text{sqrt}(gh)$, such that the resonant time scale is modified (increased). Moreover, resonance with friction is broad-band, and 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).

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.

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).

“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

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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 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.

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

“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? Not sure the model framework can look into this (see earlier comments)

“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 such as Lee et al., 2017 and Ross et al. 2017 (and Du et al. 2018).

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

“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 gradient induced circulation and transport.

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.

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.

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

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, under sea-level rise scenarios. See also again the Talke & Jay 2020 review for discussion on and references for the “inlet choking effect”.

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?

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2019-50>, 2019.

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