



EGUsphere, author comment AC1  
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## Reply on RC1

Pia Kolb et al.

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Author comment on "Modelling the impact of anthropogenic measures on saltwater intrusion in the Weser estuary" by Pia Kolb et al., EGU sphere,  
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*Dear referee #1,*

*thank you for taking the time to read our manuscript, and for your detailed and valuable feedback. We highly appreciate your inputs, which helped us to hopefully substantially improve our paper. Please find below our responses to your comments (in italics).*

The manuscript presents results from a modelling study of how changes in bathymetry of the Weser estuary affect the salinity intrusion. The study evaluates four time periods and calibrates them separately to account for variability in data availability. The main result is that dredging has resulted in a modest increase in the salinity intrusion, but this is only apparent using comparable forcing conditions because the mean change in salinity intrusion is much less than the natural variability with river discharge.

Overall the manuscript is well written and contains appropriate citations. Separately calibrating the different time periods is a distinctive approach which makes sense if there are data available to support the model evaluation, but that may not always be the case. The results presented here are broadly consistent with similar studies, and it could be a valuable contribution to the literature on the topic of anthropogenic modification of estuaries. However, I have a couple of general comments on how the analysis could be clarified to examine the physical processes more substantively and make the results more broadly applicable.

- The approach of calibrating to multiple time periods results in different optimal friction parameters among the different periods. This is attributed to differences in along-estuary depth variability (Table 1), which is explained by differences in bathymetric data resolution. The differences among most of the years are quite modest (5-20%), and are much smaller than the "additional roughness" imposed to represent tidal damping (line 258) or the variation in the effective roughness (Table 3). Additional analysis explaining this conclusion would be helpful. In particular, it's not clear how the variation in bathymetric data density (e.g., soundings from hydrographic surveys?) compares with the model grid resolution. The grid resolution is 50-250 m, and so the bathymetric data resolution would have to be lower than this to influence model

results. It's also not clear whether the difference in effective roughness may instead be due to differences in sub-grid scale roughness, which is not represented directly in the grid regardless of the bathymetric survey density. The model uses a roughness parameter based on sediment characteristics (grain size), and this does not change among the years. There are certainly examples from other modified estuaries where changes in bed sediment characteristics directly affect the friction, and that would be a different mechanism than is suggested here. For example, fine sediment accumulation in the Ems reduced friction and amplified the tides (Winterwerp et al 2013), or removal of sand waves by dredging in the Columbia reduced friction and amplified the tides (Jay et al. 2011). Perhaps it is impossible to isolate the reason for the differences in calibration values among the periods, but a more thorough explanation of the proposed mechanism and assessment of alternative explanations would build confidence in the results. See below for related comments.

*In order to answer your question, we have to briefly go into details of the hydronumerical model. When constructing the numerical grid for the UnTRIM model a so-called volume preservation is used which ensures that the water volume in the model agrees exactly with the average depths of the bathymetric data. Therefore, even subgrid information enters the model when the volume is concerned. This is different for roughness. In nature, roughness can be seen as the sum of grain and form related roughness. Grain related roughness is almost negligibly small and can be neglected for the discussion here. Form roughness which in turn can be seen as the sum of the effects of ripples, mega-ripples and dunes is represented in the model in different ways. Ripples and mega-ripples are modelled based on a roughness predictor taking into account grain sizes and flow regime. The effect of dunes could not be reliably modelled based on the predictor but had to be prescribed as additional roughness. The amount of additional roughness in turn is depending on grid resolution and bathymetric data. Dunes are to some extent depicted by the numerical grid – if present in the bathymetric data. In the examined years prior to 2012 bathymetric data almost did not resolve dunes. However, even for the year 2012 with dunes represented in the bathymetric data, the numerical model represents dunes only to a limited amount as the model resolution is coarse compared to the wavelength of the dunes. We edited Sections 3.2 and 3.6 and hopefully, it is clearer now.*

*We agree that the differences in depth resolution between 1966, 1972, and 1981 are small and might not explain the differences in the roughness settings between these models, which are determined in the calibration. We think that particularly the differences in depth resolution in the Lower Weser between 2012 and the other models can have a clear effect and need compensation. However, we decided to omit the analysis of the depth variation (former Table 1), because it seems to be rather confusing than helpful and can be easily misinterpreted. Instead, we now illustrate how bathymetrical data with different resolutions are represented in the model topographies with images of sections from two different model topographies (see Fig. 2b and c), which we describe in Section 3.2.*

- Part of the analysis examines differences (which are small) in the exponential regression between salinity intrusion and river discharge. This has been done in previous studies, and presumably is meant to be linked with analytical expressions for salinity vs discharge based on different assumptions about the dominant salt flux mechanisms. For example, salt flux dominated by the estuarine exchange scales with  $Q^{-1/3}$ , whereas tidally dominated salt flux scales as  $Q^{-1}$ . Explanation of this in the text would help readers understand the motivation for examining the regressions. However, the idealized analytical frameworks don't necessarily apply to real estuaries with spatially variable bathymetry, and instead estuary shape can have a dominant influence on observed relationships with discharge. This makes it hard to link temporal

variation in the exponent for Q to changes in salt flux mechanisms. Instead, it would be better to calculate salt fluxes directly in the model and quantify changes (if any) in the decomposed components (estuarine exchange vs tidal correlations). This could be particularly interesting for this system, as there is the suggestion that tidal processes are important for salt flux because the results mention that the salinity intrusion is greater during spring tides than neap tides. It likely also would require examination of the changes in tidal amplitude over time to aid in interpretation of the salinity dynamics. A previous study of the Hudson is referenced on changes in salt flux processes with dredging, but the Hudson is dominated by the estuarine exchange and not tidal salt flux. Deepening and reduction in friction with dredging generally increase tidal amplitude, which would tend to decrease the estuarine exchange flow but increase the tidal salt flux. The Weser could be an interesting and contrasting example to the previous studies as how the salinity intrusion responds to dredging in a tidally dominated system. It would be nice to see this aspect of the results explored in greater detail.

*Thank you for pointing out the missing link between our regression analysis and analytical models, which we included in the manuscript now. We did so after considering the alternative, to omit this analysis completely. We think that this straightforward analysis allows to grasp easily the changes in saltwater intrusion taking into account variable river discharge.*

*Following your suggestion, we included a more complete analysis of the salt flux components for a representative cross-section. Our results confirm that the Weser is dominated by tidal salt flux, not by estuarine exchange (see Section 4.6.1). We added the analysis in the paper and applied the implications thereof to the other chapters, shifting the focus towards an analysis of all processes. With means of this method, further interesting questions arise that could be examined in additional analyses. As we feel this is beyond the scope of the paper, we included some of these ideas in the discussion section. We also added the important aspect that the Hudson is dominated by estuarine circulation, in contrast to Weser estuary, in the discussion of our results (Section 5).*

Additional, specific comments follow with line numbers.

[25] Perhaps "masked" rather than "outweighed" (unless there was a shift in the mean discharge that was compensating for the shift in the mean depth).

*Action: We changed "outweighed" to "masked".*

[48] "other drivers" of what?

*Action: We changed it: "Salt transport is also controlled by"*

[76] "Ems has successively become finer..." In the other cases (Hudson, Seine, Danshui), were changes in bed composition coincident with the changes in depth? If not, is there still need to recalibrate, presuming similar numerical grid resolution and

bathymetric data (sounding density, which is the often case in systems with a long history of navigational use)? Could this include changes in bedforms with dredging (removal of sand waves)?

*Reply: Even presuming an ideal data coverage and data quality, there would still be a need to recalibrate as roughness predictors are unfortunately still not perfect. They could hopefully significantly reduce the amount of calibration required, if changes in bed composition can be reliably determined. Changes in bedforms, such as a removal of sand waves, will probably not be feasible at subgrid level in the foreseeable future, roughness predictors usually assume an undisturbed system. Possibly these effects can be considered in roughness predictors but even then, some amount of calibration will be necessary.*

*Action: We included a discussion in which case individual calibration might be needed and in which case not (see Section 5). Furthermore, we describe in more detail how bathymetrical data and bed forms are represented in the model topographies (see Section 3.2 and 3.6).*

[78] The bathymetric data resolution has to be considered in conjunction with the bathymetry data and the model grid resolution. Is the assumption here that the grid resolution exceeds the bathymetry data resolution? Is that often the case? It seems like it would not be the case in most estuaries with data collected for navigational charts.

*Reply: In our case, the data resolution for historical topographies was similar or lower than the grid resolution especially in the area of Lower Weser. This might also be the case for other estuaries with respect to historical topographical data.*

*Action: We added information about the type and resolution of topographical data in Section 3.2. We further changed "with an effect on form drag" to "which can have an effect on form drag", to indicate that there is not necessarily an effect – it depends on the respective resolutions.*

[80] "different calibrations..." Does this presume similar availability of observations (water level, salinity) for calibration? Is there a tradeoff where diminished availability of historical data to use in the calibration would degrade the confidence in the historical calibration to the point that it would be better to use the more robust and data-constrained modern calibration?

*Reply: Of course, there are cases when an individual calibration does not make sense.*

*Action: We adjusted the text and included the aspect in our discussion (Section 5): "It has to be noted that calibration is only possible if observational data are available to support a robust calibration. Recalibration with imprecise data of low resolution might even negatively affect model results. In addition, it might not always be necessary, depending on the representation of roughness and sediments, and the resolutions of bathymetric data and grid."*

[89] "impact is larger..." impact of what?

*Reply: This refers to the non-linear relationship between discharge and saltwater intrusion – for example, an increase from 100 to 200 m<sup>3</sup>/s would induce a stronger shift of the*

salinity front compared to an increase from 800 to 900 m<sup>3</sup>/s.

*Action: We changed the sentence to make it clearer: "... whereby the impact of discharge variations is larger for low-discharge conditions than high-discharge conditions."*

[92] Are "anthropogenic measures" just dredging or does it include other modifications (shoreline reclamation/hardening, discharge regulation, etc)?

*Reply: Between all examined scenarios, deepening measures have been conducted. In addition to that, there were some other comparatively small interventions which we describe in Section 2.2. We examine the overall effect of variations in topography (which also includes natural variations), as represented in the different topographies. Based on that, we derive conclusions on the effect of deepening measures, which most strongly contribute to the topographical variations between the scenarios.*

*Action: We added "in particular deepening measures".*

[123] "between Brake and Bremen..." Note distances from the mouth for context and to help with the interpretation of Fig 2.

*Action: We included it in the manuscript.*

[143] What are the values of the horizontal viscosity and diffusivity?

*Reply: The model is set-up with constant viscosity (0.1 m<sup>2</sup> s<sup>-1</sup>) and diffusivity (0.1 m<sup>2</sup> s<sup>-1</sup>) for all experiments.*

*Action: We included the information in the manuscript.*

[164] "data were scarcer..." Can this be quantified by comparing the bathymetric sounding data density (if that's what was used) to the grid resolution for the different eras? It could be a range if spatially variable, but would be helpful for context particularly given the importance placed on the data density.

*Action: We included more detailed information on the type and resolution of the bathymetric data which were used in Section 3.2.*

[165] "the resolution of small-scale features such as bedforms in the different model topographies is not directly comparable". It seems like most bedforms aren't represented in the model grid with resolution of 50-250 m. For example, sand waves that are 1 m high and a steepness of 0.05 have a wavelength of 20 m. Perhaps clarify what is meant by "small-scale" and what types/sizes of bedforms?

*Reply: Maybe the term "small-scale" was misleading.*

*Action: We omitted the term "small-scale" and added detailed information on how bed*

*forms are represented in the model topographies (see Section 3.6).*

[177] "Minor differences in depth variation do also occur between the individual model topographies" What does this mean?

*Reply: We wanted to say that there are small differences in depth resolution between all model topographies, i.e. 1966, 1972, 1981, 2012 (see Table 1). Not only between historical topographies vs. 2012.*

*Action: We decided to omit the analysis of the depth resolution, because it seemed not to be helpful (see above).*

[178] "the different resolutions of the underlying bathymetric data." As noted above, it'd be useful to quantify this directly and report it relative to the model grid resolution.

*Action: We added the information in the second paragraph of 3.2.*

[Table 1] The listed differences in roughness do not seem large – it seems like other than 2012 in the seaward section the values are essentially the same. Are the differences in roughness significant compared to the other sources of uncertainty in the data and model? This could be tested by smoothing the bathymetry to be comparable to previous years and seeing how the calibration changes.

*Action: Within the reviewing period, we did not have time for additional simulations, but we included the idea in our discussion. With regard to the depth variation, we decided to omit it (see above).*

[191] It seems like changes in the sediment composition could be a bigger effect than the small changes in the variance at the grid scale listed in Table 1. If dredging is removing sand waves or leading to finer bed sediment with increased deposition, then this would reduce the bottom roughness. Perhaps that has the same effect as is being implied by the changes in grid-scale roughness? The grid scale roughness of  $\sim 0.15\text{-}0.3\text{m}$  variability over 50 m (Table 1) gives a bedform steepness of 0.003-0.006, which is much less steep than the sand waves and other bedforms that would be subgrid scale and could dominate the roughness represented by  $z_0$ .

*Reply: We agree that the effect of changes in sediments and bedforms on calibration results can be equally important compared to the effect of different resolutions of topographical data.*

*Action: We deleted the sentence: "We assume that the effect of this simplification is small." We rewrote Section 3.6 to make it clearer and added information how bed forms are represented in the model topographies.*

[212] Did you quantify the "error" in the neural network salinity by comparing the model results to the observations? That could provide some context for interpreting the

uncertainty in the periods without observations at the boundary.

*Action: We added the information in the manuscript.*

[245] "An increase in potential energy anomaly indicates an increase in stratification..."  
Clarify that this assumes a constant water level  $\eta$ .

*Action: We added it in the manuscript.*

[257] Is the additional roughness spatially varying? Is it related to a physical parameter (e.g., mean depth, grain size, or distance along the channel, etc) or is it allowed to vary independently?

*Reply: The additional roughness is spatially varying, it was defined during calibration to best possibly represent the damping of the tidal wave along the estuary. It is not related to a physical parameter.*

*Action: We added more information in the text: "The additional roughness increases from Weser-km 55 towards Weser-km 26 and decreases afterwards again. In this way, damping of the tidal wave could be well represented."*

[260] Are the dunes and ripples not represented in the model grid because the van Rijn formulation does not match up with observed ripple size/steepness? Or is it that the grain size in the model does not match that in the real world? Please clarify.

*Action: We now describe this aspect in Section 3.6.*

[270] Omission of tributaries is mentioned as a reason for additional roughness, but is the presence/absence of tributaries different among the cases? If so, it seems that would not explain differences in the calibration among the periods?

*Reply: Mainly, we think that the omission of tributaries can explain why an additional roughness had to be defined in the Lower Weser for all scenarios. In combination with other factors (for example inaccuracies in topographical data), it could also strengthen model inaccuracies in individual models.*

*Action: We rewrote parts of Section 3.6 and hopefully it is clearer now.*

[295] The result that the salinity intrusion is farther landward during spring tides seems notable, in that in many other systems where salinity has shifted with deepening the gravitational circulation dominates the landward salt flux, so salinity intrusion is maximum during neap tides. Perhaps this will be addressed in the discussion...

*Action: We included the aspect in our discussion.*

[342] The difference in exponent may be statistically significant (i.e., unlikely to be due to chance), but it is still really small (0.164 vs 0.145), so it perhaps it's not so important? Any difference in salt flux mechanisms associated with that small a change would be subtle, and it'd be hard to ascribe the differences in transport processes to the exponents alone. If that topic were of interest, diagnosing the salt flux mechanisms in the model directly would be the way to do it.

*Reply: We agree that it was unclear in the text whether the difference in exponents can link to differences in processes or not.*

*Action: We adjusted the text to clarify that, in our view, the difference is too small to allow for such conclusions. Instead, we analyzed salt flux mechanisms in Section 4.6.1.*

[342] This p-value should be reported as ( $p < .001$ ) since it is not exactly 0 but rather 0.000 to three decimal places.

*Action: We changed it in the manuscript.*

[387] "saltwater intrusion length is similar in the four scenarios." While the differences in salinity intrusion are smaller than in the case with different roughnesses, the trends are similar (1972 and 1966 similar and shorter than 2021 and 1981), and it seems inaccurate to say the four scenarios are similar.

*Reply: The four scenarios are not similar, but the trend is not as clear compared to the simulations with recalibrated models.*

*Action: We changed it in the manuscript.*

[390] "less energy dissipation..." It would be valuable to diagnose this directly with analysis of the differences in tidal amplitude. Presumably, the roughness affects the tides, which then affects the salt transport. Showing the "why" of this response would clarify the physics and make it more transferable to other systems.

*Action: We added in the manuscript: "simulations with identical roughness settings do not adequately reproduce tidal energy, e.g. the tidal range in 1981 is larger than in 2012 contrary to observations. The erroneously increased tidal range exceeds the effect of changes in other processes, so that the limit of saltwater intrusion shifts seawards, and not landwards, between 1981 and 2012."*

[414] "As expected..." Is it expected because of the river discharge? It'd be worth stating directly.

*Action: We added it in the manuscript.*

[417] "Through the increase of the landward relative to seaward volume transport and river discharge, saltwater intrudes further into the estuary." It's not obvious that this

must be the case. If the volume transport scales with the tidal velocity and the difference between landward and seaward volume transport scales with the river discharge, then increasing the tidal velocities can have the opposite effect of reducing the estuarine circulation and mean stratification, thereby reducing the salinity intrusion. The salt transport in the Weser may be predominantly due to tidal processes rather than the mean circulation, but this should be explained, and the changes shown.

*Reply: Thank you for pointing out the ambiguity in our interpretation. Based on the analysis of the salt fluxes (Section 4.6.1) we can now confirm that transport in Weser estuary is predominantly dominated by tidal processes.*

[443] “Therefore, the small difference between 1966 and 1981 cannot be clearly attributed to a change in salt flux mechanisms.” This is repetitive of a previous section, and if the question is about changes in salt flux mechanisms then that should be analyzed in the model results directly.

*Action: We combined the paragraph with Section 4.3 to avoid repetition and make the paper more concise.*

[460] “However, models representing historical system states are often not calibrated” In the citations here, Ralston and Geyer compares their model with historical salinity data (Fig 6 and Fig 7), and Grasso and Le Hir and Liu et al. calculate model skills for their modern model configurations. Perhaps this should be recast to say that recalibration is only necessary 1) if the model grid does not capture changes in roughness that affect model skill, and 2) there are observational data to support a robust calibration?

*Action: We agree and included the aspects in our discussion.*

[463] “We recommend...” Related to the previous comment, it is usually a lack of observational data that limits historical model calibration. What's the recommendation in that case? How do the "artificial surface irregularities" suggested to compensate for low resolution data relate to changes in topography at larger scales by dredging? One could imagine the irregularities being bigger (deeper channel=bigger sand waves) or smaller (maintenance dredging planes off big sand waves).

*Action: In our discussion, we included some ideas when calibration might be needed and when not. The sentence about artificial surface irregularities is now in Section 3.6, we combined some sections to make the text more concise and avoid repetition. To clarify, we changed the sentence: "Alternatively, the low resolution of historical bathymetric data could be compensated by adding artificial surface irregularities, which represent the depth variation due to e.g. sand waves, as implemented by Hubert et al. (2021)."*

[486] What are the “analytical descriptions”? Is the suggestion that the variance from the power-law is greater for the Weser than for other systems? Or does it refer to the difference between -0.28 and -0.15 for the Hudson vs the Weser? That could reflect differences in transport processes and bathymetry between the systems, but does not necessarily mean that either is more or less "limited" in using this approach to characterize the relationship between discharge and salinity.

*Action: We changed it in the manuscript: "The large difference from the sensitivity found in our study could be related to differences in bathymetry and transport processes between the estuaries. While saltwater intrusion into the Hudson is dominated by estuarine exchange flow, we show that the Weser is dominated by tidal pumping (see Section 4.6.1)."*

## References

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