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Earth Surface Dynamics Discussions

Interactive comment on "Morphological effects of vegetation on the fluvial-tidal transition in Holocene estuaries" *by* Ivar Lokhorst et al.

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Reviewer 1

We thank anonymous referee 1 for detailed comments that helped to clarify parts of the manuscript while the general comment in the end pointed to an important omission in our discussion.

The major comment was that the model limitations and potential effects of missing processes could be better explained. We added several sentences to specify the potential effects of missing processes. Salinity and biochemistry have quite some implications for the distribution of species along estuaries. However, it is not yet known whether characteristic salt marsh species along the salinity gradient have different eco-

engineering traits with significantly different effect on the long-term morphodynamics. Our model is a new tool that, in further research, may provide valuable insight in such patterns emerging along the estuary. The more important simplification in this first model study is, however, the absence of waves. It is well-known that waves attack the marsh edge, resulting in steep cliffs and salt marsh retreat. As the effects of waves are likely to be limited in the central estuary compared to the outer estuary due to fetch limitation, their absence may lead to an overestimated effect of vegetation on large-scale morphology. On the other hand, the difference between waves in the mouth and further landward would enhance our modelled vegetation distribution along the estuary. Moreover, we expect larger differences between presence and absence of vegetation and mud than between different marsh species that occur along the salinity gradient. We now discuss this in the revised manuscript (supplied with tracked changes as supplement).

We adopted all other textual and technical comments. Two of the specific comments require a longer answer.

Referee comment: Page 3 Figure 1. It is not very clear how the authors determine the "tide dominated", "mixed energy", and "river dominated" zones? Is there an objective way of doing this? There is no exact objective way of classifying a continuum. We used visual interpretation of the patterns we observe in the landscape, including large scale meander bends, the presence of multiple parallel (sub-) tidal bars and the absence of intertidal morphology, notably creek patterns that form only in significant tidal influence in the mixed tidal-fluvial zone. The gradual transition from mixed to tide dominated was mainly based on the transition from fluvial meanders and chute bars towards tidal bars with indications for bidirectional flow in meander shape, landward-facing barb channels and mutually-evasive ebb- and flood-dominated channels. For the Dovey the identified transitions were verified by actual measurements of tidal range and flow velocities. We added one sentence with this explanation in the caption with a reference to Dalrymple et al., 1992, which we used to identify define change in dominant energy types from

morphological changes along the estuary. Referee comment: Page 10 Table 2. The model considers the marsh species “Spartina Anglica”, while the model parameters are chosen based on “Spartina Alterniflora”. Albeit the exploratory nature of the model, I think it is still good to use common parameters based on Anglica, which would be more convincing.

We fully agree that it would be good to use root parameters based on Anglica. However, we did not manage to find values for Anglica’s root length development as a function of time. Therefore, we decided to take the root development of its closest relative. Moreover, we noticed in the modelling that the most important cause of mortality is inundation duration, making our conclusions insensitive to this assumption.

Reviewer 2

We now thank Eli Lazarus in the acknowledgements for the useful and constructive review.

The first main comment is a suggestion to expand the model-based dataset to compare the effect of vegetation and mud on different initial bathymetries. We agree that additional model simulations with different initial bathymetries would indeed strengthen the research and illuminate the comparison with the natural estuaries, which also started with different bathymetries. However, this requires a lot of additional results description and methods to fully explain what we did, and partly shifts the focus from the effect of introducing vegetation (and mud) towards the effect of vegetation and mud plus initial estuary bathymetry. It would even be better to let the model develop different bathymetries due to different boundary conditions but the referee clearly understands the inhibiting computational costs of this. However, the main reason why we were reluctant to run more simulations is the inhibitive run time. The simulations including vegetation can take up to 2 months for 100 years of simulation. As we run with a low morphological acceleration, even simulations without the vegetation model last more than 1 month. Following the referee suggestion involves 3-6 new initial bathymetries

which would have to be run at least with the vegetation model, but preferentially with all 4 scenarios’ (reference, mud, vegetation, mud and vegetation). Moreover, results of our earlier modelling with mud (Braat et al. 2017) strongly suggest that the general large-scale development would be fairly similar in pattern. We therefore focus on the present new insights and continue to develop model code and performance for future work.

The second main comment was to plot an envelope around the width-fraction of vegetation for the natural systems to obtain a sense for the variability in the data. We added this envelope to figure 13 (Figure 1 in this reply), which now shows that our findings are fairly consistent between natural estuaries. We used the 20th and 80th percentile and plotted this around the modelled data which shows there is definitely some variability, also due to a spreading in where the BLCZ occurs, but the main trend remains clearly visible.

A related question, also with respect to Figure 13, was about the range of mixed energy zones. We would like to stress that the energy along the estuary is a continuum and there is no clearly defined mixed energy zone. However, we agree that it would improve the readability and strengthen the message when we point out the rough location of this zone. We made estimates of the location of the bedload convergence zone by taking the approximate location where the effect of the river on the energy in the estuary diminishes. These locations where plotted as a line (range) within the figure to indicate the rough location of the centre of the mixed energy zone for the 9 natural estuaries. On top of that we plotted the 9 locations we determined to get a feeling for how the mixed energy zones of these estuaries are distributed on this line. These normalized locations are also added to table 4.

There are eight detailed comments we would like to address specifically: Referee comment: Title (and related uses) – I suggest flipping the transition to read "tidal–fluvial transition", since your physical "position zero" reference throughout the paper is the mouth of the estuary. (This switch in the terminology would propagate through the

manuscript.)

These two terms occur equally often in literature, and we agree with your suggestion that “tidal-fluvial” makes more sense with respect to our “position zero”. We adjusted this terminology through the manuscript.

Referee comment: Abstract – the first line is a bit misleading. I don’t think the question is "whether similar. . .feedbacks exist" (they do, as the authors demonstrate) but how they manifest in full-scale estuarine settings that is poorly understood.

You were right that the question is not whether these feedbacks exist, but how they affect the morphological development of the estuary. We adjusted the sentence to read: “Vegetation enhances bank stability and sedimentation to such extent that it can modify river patterns, but how these processes manifest in full-scale estuarine settings is poorly understood.”

Referee comment: Abstract, L5 – the mention of mud in a "sandy" estuary model is confusing here. Suggest deleting "sandy" for clarity. (More detail comes later in the manuscript, anyway.)

We agree that the word sandy is confusing in this part of the abstract. We added the word to emphasize that we are investigating the introduction of mud instead of starting out with a muddy (consolidated) estuary, which might result in significantly different morphological evolution. However, you are right that more detail comes later in the manuscript and therefore we removed it here.

Referee comment: Abstract, L10–15 – These sentences are confusing because the "results show" delivery bounces between the coupled/isolated/coupled results. This full bottom third of the Abstract could be revised for clarity.

The structure of the bottom part of the abstract was indeed somewhat confusing. Therefore, we restructured it to follow the same order we use elsewhere in the manuscript (reference, vegetation, mud, mud/vegetation). Also, we further emphasized

whether we are talking about model results or analysis of natural estuaries.

Referee comment: P2, L27 – This section doesn’t really constitute a review. I suggest merging it into the Introduction without making it stand apart as its own section, and make it do more work for you. In fact, the paragraph beginning at L27 is stronger than similar material that comes before it – and might easily substitute (more or less) into the very first paragraph of the manuscript.

We agree that section 1.2 did not really constitute a review and therefore we merged it into the introduction (former 1.1) after the first paragraph. We did a little rewriting, but we considered the first paragraph of the introduction to give a concise overview of the topic after which former section 1.2 goes more in depth. Therefore, we did not substitute much of section 1.1, but we did use 1.2 to strengthen it.

Referee comment: P11, L7 – This morphometry/normalisation step is an interesting one, and I encourage the authors to push it a bit further (e.g., to delineate the "mixed energy" transition, as discussed in the main comment, above).

We added information here about how we used this normalization and stress that the zones are convenient labels on a continuum (also see response to referee 1). We show the general trend in energy along the estuary, along with variability (as you suggested) and normalized vegetation distribution along the estuary which together indicate the mixed energy zone. Referee comment: P24, L6–8 – Sentence beginning "Regardless. . ." is great. But I agree with Reviewer #1 that there is an opportunity here (probably ahead of this sentence) for the authors to briefly summarise what potential (albeit secondary) effects waves and these other environmental factors might have. (Push the mixed energy node up or down the estuary? Widen/narrow the vegetation fringes?)

We agree with your suggestion as well as with reviewer #1 to summarise the effect of other environmental factors. We wrote an additional paragraph with model limitations and simplifications where we discuss the processes we ignore and how these poten-

tially affect the results. An extensive explanation is given in the response to reviewer #1.

P24 – Manuscript ends abruptly with a set of very fine-scale conclusions. Suggest the authors attempt to zoom out to a wider scope of consideration here and frame the implications of these findings, as they do at the end of the Abstract. We followed your suggestion here and added a small paragraph at the end of the conclusions with a wider scope of consideration and implications. We emphasized the potential infilling of estuaries on centennial timescale due to infill of the central estuary and the resulting reduction of tidal prism. We adopted all other specific comments in the revised manuscript. The restructuring of parts of the introduction and parts of the methods helped to increase readability. âĂČ

Reviewer 3

We thank anonymous referee 3 for helpful and useful discussion. Five main comments were made that we discuss below. Minor comments were all adopted.

The first comment is that we use various terms for marshes including tidal marsh and saltmarsh, of which the relation with the tide-dominated and mixed-energy reaches is unclear. We agree that this needs better clarification. We mean marsh in general. The model has a generic marsh species and while future modelling will include waves and salinity, we cannot strictly separate salt marsh from freshwater marsh and tidal marsh from other marsh (also see response to Referee 1). We now use the general term tidal marsh because tides are the main driver for marsh settling and mortality. We now also explain that the zonation in the estuary is useful terminology for what really constitutes a continuum, and define that here we mean the seaward, middle and landward part of the model domain spanning the tidal river to the estuary inlet. We now clarify our choice for a generic marsh species at the end of the first paragraph in the introduction and the differences between the zones at the end of 1.2, which will move up based on suggestions of reviewer #2.

The second comment was about whether it is possible to represent saltmarsh vegetation with a riparian vegetation model. The point of that riparian vegetation model is that it includes the basic vegetation life stages and the basic eco-engineering properties shared by many plant taxa. We use the similarity between riparian and salt marsh vegetation to modify the model for our specific needs, particularly the properties of life stages of settlement, growth and mortality due to currents, uprooting and flooding. These variables were parametrized with appropriate thresholds for marsh vegetation. The main hydromorphological effect of vegetation in the model is flow retardation as a function of stem height, diameter and stem density, which is independent of the type of vegetation. The key modification in the riparian model is the recording of tidal inundation and an appropriate choice of ecological timestep related to the tidal period. As we maintain the essentials of the riparian model this approach also means that we will be able to model the entire river continuum in the future with a number of different species. We added several sentences with explanation at the beginning of the methods section to explain this.

The third comment is a request for clarification of which model components were incorporated in the Delft3D code in section 2.1. We thank the reviewer for pointing this out and added several sentences at the beginning of section 2.1 that state that the equations in section 2.1 are all implemented in Delft3D, and were either default or activated by us. The entire vegetation administration, however, was done in Matlab.

The fourth comment asked the basis for the 2 week ecological timestep we applied. As in our riparian vegetation modelling, the ecological timestep is a compromise between the computational cost of the vegetation model and the change in eco-engineering properties in that period. We know from the growth functions that the marsh species has no appreciable change in size over the two week period. The problem is analogeous to that of modelling morphological change which is also much slower than the flow, allowing the use of the morphological acceleration factor. While plants have the fastest shoot growth after settlement, it takes much more time for the cover to grow

denser and it is the mortality that determines where the vegetation can settle over times long enough to modify the flow pattern and the resulting morphology. In the manuscript itself we added 1 sentence to clarify that changes in species size over 2 weeks are small and that this balances with the increasing computational demand for smaller timesteps.

The fifth comment is a request for plots showing mud and vegetation cover in the central zone of the estuary for different simulations and we agree that a cross-sectional representation provides a much clearer illustration of the increased vegetation and mud cover in general, and specifically on the tidal bars. We will add two plots with cross-sections at 2 locations in the estuary to visualize the increased vegetation and mud cover.

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2018-29/esurf-2018-29-AC1-supplement.pdf

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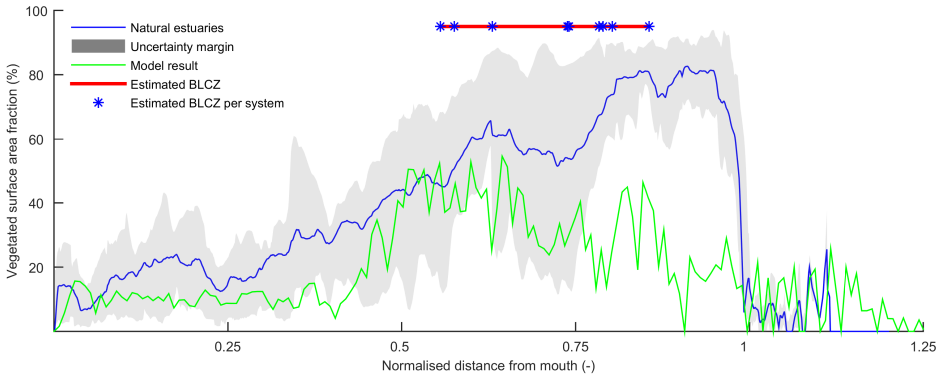

Fig. 1. Relative vegetated width along the estuary averaged for nine natural estuaries compared to the simulation with mud and vegetation.