Reply to Anonymous Referee #3 comments on "Analysis of the drainage density of experimental and modelled tidal networks"

5 December 22, 2013

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10 We are grateful to the anonymous reviewer for the constructive comments which will definitely result in a better manuscript. The reviewer's comments have been addressed below point by point *in italic fonts*.

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Comments from the reviewer:

This is an interesting paper with useful analysis of different methods yielding some insight into the accuracy of the analysis of the drainage density of experimental and modelled tidal networks techniques. However, I think there are some uncertainties in the present version, which could affect the conclusions. In addition, the discussion of the characteristics of the different used method could be integrated based on the fact that some delft3D issue could not be known.

(1) To investigate the question of model sensitivity to various modelling techniques for scaling or aggregation of landscape attribute, it is necessary to work within the context of a given model's data requirements and sensitivity. Model sensitivity to input data error propagation can be evaluated to specify the form and acceptable limits of accuracy of input data sets describing land surface attributes. For certain morphological processes like the tidal one that are strongly dependent on soil water status and also on elevations, much of the process variations at sufficiently large spatial and temporal scales can be explained with direct measures. In the experimental part what are the parameters that are taken in consideration considering the very different sites the authors are referring to? Much of the safe calculated channel development could be highlighted using some quantitative method to show how the two channel network are linked?

Response: We wish to point out that the focus of our investigation is the use of drainage density a parameter used to gain insight into experimental and numerical tidal networks. The sensitivity to parametrizations is explored elsewhere (Zhou et al., 2013) while the sensitivity to initial seabed configurations (e.g. different perturbations) deserves a thorough separate investigation (see also our reply to comment #1 of reviewer #2). Our model simulations refer to real cases only in very general qualitative sense and the link between field-lab-model is only made in terms of the tidal prism, i.e. we do not address other critical components of the system like,

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for example, sediment characteristics, river inflow and overtides. In our contribution we use drainage density and hypsometry (added in the resubmitted version of the manuscript) to compare channel networks. Other parameters have been used in other comparisons (Zhou et al., 2013) and they all indicate close similarity between experimental and numerical tidal networks. Overall, we have added the following sentences in the Discussion section:

"...<u>The existence of feedback mechanisms between various landscape-forming</u> processes (e.g., tides, waves, biological effects, sea level rise, human intervention) limits our understanding of the ontogeny of tidal networks (Rinaldo et al., 1999; Coco et al., 2013). By reducing the number of processes affecting the evolution of tidal networks, controlled laboratory experiments provide a good base to gain insight into these systems. While tidal flows are the external driver of bathymetric changes, the slowly evolving estuarine bathymetry can also constrain the tidal flow and determine flow patterns. Therefore, estuarine landscapes are the result of the mutual adaptation between flow and morphology (i.e. morphodynamic feedback)."

(2) The paper is very short in some part (see the 2 Methods paragraph divided in two sections). It is obvious that the authors have made a lot of work on that and it is not possible to present this entire work in details to a journal paper. But paper is good when it is clear and concise even if you show some more details of previous related work. On the opposite the discussion is starting with some introduction that could be very useful at the beginning of the paper (as already stated by another review).

Response: The reviewer is correct. We actually put a lot of effort into setting up the models, particularly in terms of choosing proper values for some parameters (e.g. time step, morphological factor, bed slope terms and friction coefficient). Sensitivity tests were carried out to determine those values. We did not include this in the manuscript because the focus was drainage density of tidal networks and a different manuscript specifically focusing on model sensitivity to parameterizations is under review (Zhou et al., 2013). As for the Discussion part, we will rewrite and move some sentences to the Introduction section as also suggested by the other reviewer, as follows: (a) merge part of the first paragraph in the Discussion section to Introduction section where it fits better; (b) include a clearer definition of equilibrium concept as used in this manuscript; (c) add a figure of hypsometric curve as another metric to investigate the differences between simulated and laboratory networks. We refer the reviewer to the new version of manuscript.

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(3) I am quite enthusiastic about the objectives of the paper and the methods used but I feel a little bit lost at one paragraph: the Discussion is not always effective. I would like to see it draw upon not only on the geomorphology literature and some of the recent literature on the physics of flow to rationalize the results in a mechanistic manner. Do the authors attempt to understand their results through a consideration

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of physical mechanisms, or do they just compare their conclusions with alternative experimental (e.g. they may find correlations between drainage density and marsh elevation as already stated in some of their previous papers, which would provide a link to physical processes). Such a description would maximize the impact of the manuscript in the fields of the geomorphologist.

Response: In the new version of the manuscript, the perspective of physical mechanisms will be clearer (we will open the Discussion section specifically pointing at the morphological feedback and how it shapes and constrains estuarine evolution). We need to point out that during the laboratory experiments no flow measurements were collected so that a detailed comparison was not possible. Also, it is worth reiterating that the model we adopted to calculate the drainage density is actually based on a flow model (Rinaldo et al. 1999). We did not include vegetation effects in either physical or numerical models, so that at present we can not link changes in drainage density to eventually present vegetation patterns. Work is under way (both numerically and experimentally) to address this limitation.

In our new manuscript, we have largely modified the Discussion (also by introducing a figure). We hope this response addresses the comment of the reviewer.

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References cited in this reply:

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