Reply to Anonymous Referee #2 comments on "Analysis of the drainage density of experimental and modelled tidal networks"

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Authors: Z. Zhou, L. Stefanon, M. Olabarrieta, A. D'Alpaos, L. Carniello, and G. Coco

We would like to thank the anonymous reviewer for the insightful comments which will definitely result in a better manuscript. The reviewer's comments have been addressed below point by point *in italic fonts*.

Comments from the reviewer:

In this paper, the authors compare the results of previous experimental and numerical modeling of tidal networks, particularly in terms of drainage density, here measured through the exceedance probability distribution of unchanneled length. The topic is of interest, particularly from a 'metrics' definition point of view. I also appreciate the coupled experimental-numerical approach to understand the morphology of a system such as tidal networks. The definition of common testing metrics among the two is a valid contribution in itself. I think the paper is sound and although quite narrowly focused on drainage density (it could be expanded to other metrics) represents a good contribution.

There are several aspects where I believe the manuscript could be improved. I point at some of these aspects below:

(1) First of all, there needs to be more clarity on the definition of 'equilibrium' and 'stability'. Are we talking about 'frozen' networks or a statistical equilibrium? I think this is important also later when different morphologies obtained from different initial bathymetries are compared.

Response: *We agree with the reviewer that 'equilibrium' and 'stability' have not been well defined in the manuscript and we will make this clearer in the revised version (in the Discussion section) to avoid any confusion. As discussed by many authors, a "frozen" or "static" equilibrium state (strictly null sediment fluxes and no bed level change) can hardly exist in reality also because external forcing is constantly changing. A different definition, sometimes indicated as "dynamical equilibrium", is the one characterized by null gradients in sediment fluxes (and no bed level change) over a specific time scale. The equilibrium (or stable) state considered in this manuscript is dynamic because gradients in sediment fluxes, and for continuity bed level changes, approach zero even though sediment fluxes are never strictly null. In our understanding, statistical equilibrium is slightly different as* *statistical equilibrium implies that small fluctuations in the system do not necessarily cease even if the overall property of the network remain unchanged.*

In our study, the basic structure of the tidal network took shape after 35 yr. However, we did not consider this as the equilibrium state since there was still considerable deepening of the channels and net exporting of sediment from the lagoon. Instead, we indicated that equilibrium was approached after approximately 200 yr in the manuscript (see Page 7, line 15-16). Morphological changes over one tide almost vanished in terms of both horizontal expansion and vertical deepening. At the same time, the net sediment exchange between the lagoon and the outer sea approached zero. Since the characteristics of the network do not change over time in terms of channel position or distribution of intertidal areas, the system seems to be in a dynamical equilibrium rather than a statistical one.

Overall, we will add the following sentences in the Discussion section:
"... <u>In this context, it is important to specify that the equilibrium (or stable) state considered in this study is not a "frozen" or "static" equilibrium state (strictly null sediment fluxes and no bed level change) which can hardly exist in reality (especially if one considers that external forcing is constantly changing). Instead, a different definition, sometimes indicated as "dynamical equilibrium" and characterised by null gradients in sediment fluxes (and no bed level change) over a specific time scale, is more appropriate to describe our numerical simulations. In fact our results indicate that the gradients in sediment fluxes, and for continuity bed level changes, decreased over time and approached zero after approximately 200 yr, even though sediment fluxes were never strictly null. Our definition of dynamic equilibrium is also different from the usual definition of statistical equilibrium which implies that small fluctuations in the system do not necessarily cease even if the overall property of the network remain unchanged."</u>

(2) The results are said to be different, but in terms of what? Can the authors produce some quantitative results beyond the visual comparison of the resulting patterns and the exceedance probability? Is that the only metric that is different?

Response: *In the manuscript, we showed three numerical simulations: one micro-tidal case and two meso-tidal cases. We think that the reviewer was referring to the results of the two meso-tidal cases starting with different initial bathymetries (i.e. different initial random bed perturbations). These two simulations showed that even small random bottom perturbations can play a considerable role in determining the long-term morphological development of tidal networks, and hence the overall evolution of the drainage density. We did not consider other metrics primarily because the focus of this work is to investigate drainage density which was measured through the exceedance probability distribution of unchannelled lengths while other metrics (e.g. evolution of lagoon bed level change, channel width-to-depth ratios) have been discussed in detail in another paper which is currently under review (Zhou et al., 2013). However, we agree that the reviewer's idea of* *using some other metrics is worthwhile. For this reason we have decided to add a new Figure (Figure 11) which shows: (a) the detailed differences in the initial and final bathymetries resulting from numerical simulations (only different for the random seed) and (b) the hypsometric curve as another metric to quantify the general differences between simulations. Aside from Fig. 11 (shown below) we will also modify the text in the discussion as follows:*

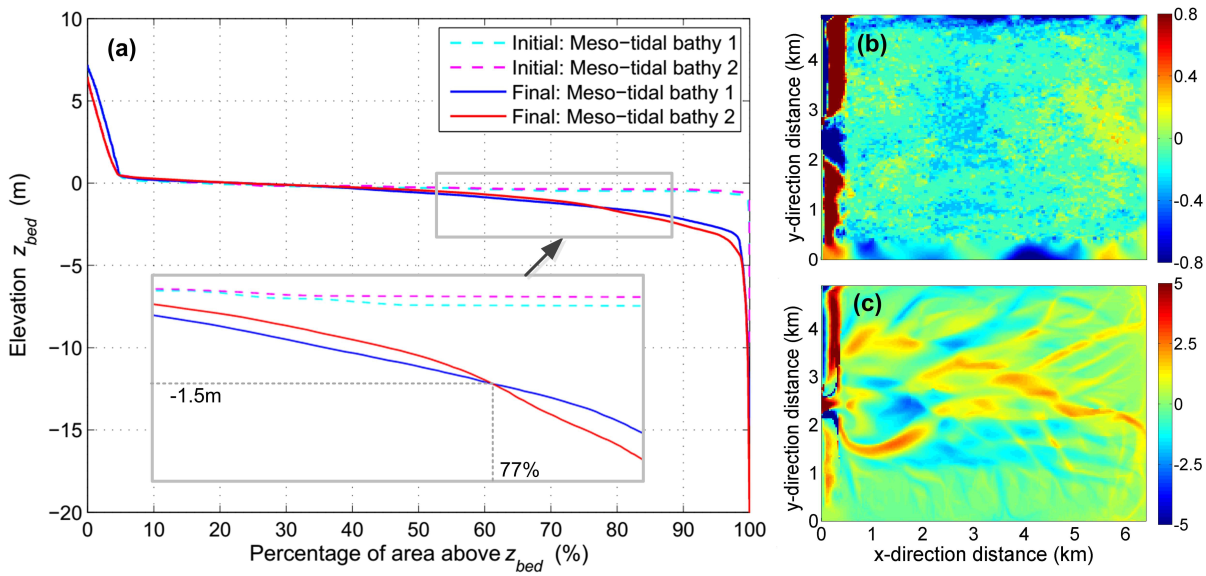

Fig. 11 (a) Initial and final hypsometries of the 1st and 2nd meso-tidal bathymetries; (b) and (c) are the Initial and final differences of bed elevation between bathymetry 1 and 2, respectively (note the colour bar scales in panel b and c are different). For bed elevation, the upward direction is positive.

"... Another metric to quantify the general differences between the resulting bathymetries are the hypsometric curves shown in Fig. 11a. The cyan and magenta dashed lines represent the meso-tidal starting hypsometries for bathymetry 1 and 2, respectively. The blue and red solid lines indicate the corresponding final hypsometries at equilibrium. These two numerical simulations started with input bathymetries characterized by different spatial random perturbations (the magnitude of the perturbations was the same) and nearly 95% of the bed elevation was around the mean sea level. The existence of higher areas near the barrier islands (the red areas in Fig. 3) accounts for approximately the remaining 5% lagoon (notice that in the numerical simulations these higher areas remained unchanged since most of that area was dry even at high tide). Due to the statistical similarity in the seabed perturbations, the two initial bathymetries (dashed lines) show similar hypsometries (the elevation of the 1st bathymetry is slightly lower). The resulting final hypsometries also share a similar morphological pattern but some noticeable differences are present. The two hypsometric curves intersect so that the 2nd hypsometry (red line) is higher than the 1st one (blue line) for areas lower than 77%

while the opposite occurs for areas larger than 77%. Overall, the shallower areas are less channelized while the deep areas are more incised (2nd case), consistent with the final simulated morphologies (Fig. 3d-i) which show that less area is drained and the main channels are deeper in the 2nd case. This can also be noted from Fig. 11b and c which are the initial and final differences between the 1st and 2nd bathymetries, respectively. The initial differences are generally within the range of -0.8 to 0.8 m while the final differences can reach 5 m with different spatial distribution of channels and tidal flats. Nonetheless, hypsometric curves could only capture a general difference between the two final morphologies due to the aggregated nature of the method. Instead, the probability distributions of unchannelled flow lengths (Fig. 9) provided a better way to investigate the differences between the two cases. Some other metrics (e.g. evolution of lagoon bed level change, channel width-to-depth ratios and the relationship between tidal prism and cross-sectional area) have been discussed in detail elsewhere (Zhou et al., 2013), and overall, they indicate close similarity between experimental and numerical tidal networks."

(3) I am also concerned as to whether two such experiments (different initial conditions) are sufficient to say anything. I understand the complexity of multiple laboratory runs, but what about the numerical runs? Can this exercise be repeated multiple times? The influence of initial conditions is important in itself and it would deserve a more thorough analysis rather than a comparison of only two results.

Response: *As said in the above reply, the primary focus of this work was to investigate the drainage density of tidal networks. We definitely agree with the reviewer that the effect of initial random perturbations itself is a very interesting topic that deserves a thorough analysis and better understanding. In this contribution, it is only presented as a possible source of variability (also observed in the laboratory experiments). We have already been working on this topic and based on the results, we think the outcome deserves to be presented as a separate paper specifically focusing on the effect of initial perturbations. Overall, we think the two topics are separate and "initial conditions" are beyond the scope of the current manuscript which primarily focuses on the drainage density measured through the exceedance probability distribution of unchannelled lengths.*

(4) I am also quite curious about the meaning of these 35 yr after which both the micro and meso-tidal systems reached equilibrium. This is again related to my point above about the need to define 'equilibrium', but it seems that this should be addressed within the discussion section of the manuscript.

Response: *We refer the reviewer and readers to our response to comment (1).*

(5) Another (minor) comment regards the intro of the Discussion section. I believe the first paragraph belongs to the introduction, particularly if the authors introduce the idea of the need for analyzing coupled lab and numerical experiments rather than delivering that message in one sentence in the conclusions as it is right now.

Response: *We agree with the reviewer and in the revised manuscript we have rewritten the Discussion part 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.*

References cited in this reply:

Zhou, Z., Olabarrieta, M., Stefanon, L., D'Alpaos, A., Carniello, L., and Coco, G. (2013): A comparative study of physical and numerical modelling of tidal network ontogeny, J. Geophys. Res., under review.