

Interactive comment on “Analysis of the drainage density of experimental and modelled tidal networks” by Z. Zhou et al.

Anonymous Referee #3

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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 morpho-

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logical 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 calculated channel development could be highlighted using some quantitative method to show how the two channel network are linked?

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).

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 author attempt to understand their results through a consideration 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.

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