

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

Anonymous Referee #2

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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: - 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

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obtained from different initial bathymetries are compared. 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? - 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. - 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. - 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.

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