Interactive comment on “Non-linear power law approach for spatial and temporal pattern analysis of salt marsh evolution” by A. Taramelli et al.

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The manuscript looks at a typical problem, the presence of power law distributions in vegetated environments, from a different perspective: why there are times when we do NOT observe a power-law in an environment where a power-law is actually expected? I admit I am intrigued by the question but I am also aware that addressing this question is not simple at all as it requires a quantitative assessment of the balance between physical and vegetation processes (which are not fully understood) over a range of spatial scales. I have read the manuscript and the comments by the reviewers, and I am in line with their assessment: the submitted version of the manuscript could certainly be improved. Specifically, I agree with both reviewers when they indicated that, in the previous version of the manuscript, the objectives of this study as well as the physical insight and the technical details were all unclear. After reading the reply from the authors I sense that almost all of the comments by the reviewers can be addressed by being a lot more detailed and precise, and that overall the new resubmitted version of the manuscript is likely to be much improved. On the basis of the replies to the reviewers, I wish to encourage the authors to submit a revised version of the manuscript. At the same time I wish to request for a specific change. I suspect that a lot of the confusion (mine and of the reviewers) about the overall goal of the manuscript is related to the “assumption” that vegetation pattern size follows a power-law relationship. I think the authors need to clearly point out that the presence of a power-law is a hypothesis. I understand that for other types of environments and vegetation types this relationship holds (e.g., Scanlon et al., Nature 2007) and that for similar environments a power law seems to hold at least over a limited range of scales (e.g., figure 4 of Schoelynck et al, Ecography 2012). But this is no definitive evidence and I am not even sure that scale-dependent feedbacks should necessarily result in power laws. There are (unvegetated) systems where the presence of scale-dependent feedbacks results in the dominance of a specific scale and I do not think one can generalize. I think the authors should clearly state right at the beginning of their work that they assume there are physical reasons to expect a power law. They should also make a stronger effort to explain from a physical perspective ‘why’ such power law is to be expected. The explanation given in the reply letter (pages C585-C586) refers primarily the scale-dependent argument (which leaves me a bit cold, it is not evidence) and even after reading the paper by Schoelynck et al. I remain unconvinced. For example, I understand why a river network is scale-invariant but I struggle to understand why sinuosity is necessarily related to patch size. I think addressing in more detail these issues is critical for the acceptance of this work which is certainly of potential interest to the community.

I look forward to receiving a revised version of the manuscript.

Best regards

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