

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

**G. Coco (Editor)**

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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

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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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