

## ***Interactive comment on “On the spatial organization of the ridge slough patterned landscape” by S. T. Casey et al.***

**S. T. Casey et al.**

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Dr. Roger Moussa Hydrologic and Earth System Science

Re: Revisions of MS# HESS-2015-59

December 4, 2015

Dear Dr. Moussa,

We appreciate the feedback that we received from two anonymous reviewers on our paper “On the Spatial Organization of the Ridge-Slough Patterned Landscape” (HESS-2015-59). In response to those reviews, we respectfully propose to submit a revised paper, with many changes. This letter documents those changes.

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Both reviewers noted the compelling scientific findings in the paper, and these will be retained. They also both noted that the paper was overburdened with ancillary information and jargon. We have worked diligently to remedy both concerns in the revised paper, shortening the total length by over 20% and removing jargon to the maximum extent feasible. We believe that the product is a clearer and more concise paper that effectively highlights the most important results.

We look forward to a decision on publication of this paper in HESS. Feel free to contact me with any further questions or concerns. As before, all authors have agreed with resubmission of this revised manuscript, and no part of the paper is published or under review at another journal.

Sincerely,

Matt Cohen Associate Professor of Ecohydrology School of Forest Resources and Conservation University of Florida

\*\*\*\* Response to anonymous referee #1

Comment: 1) I have particularly appreciated the intro of the manuscript that is well organized and provides a good overview of the topic. In particular, a number of issues are introduced and as reader would like to have comments on the significant questions posed. Nevertheless, it is my feeling that the paper does not contain a good discussion about the implications of the results. How do we use these results in restorations actions? What is the impact of human activities on the observed spatial patterns?; What are the driving factors controlling specific configurations?

Response: We agree that a thorough discussion of the implications of our results is lacking. Correspondingly, we propose the addition of the details of such implications (see lines 334-341 and 378-382 of the attached draft).

Comment: 2) It is my feeling that the length of the manuscript may be reduced. Some of the analyses presented are less relevant (significant) of others. I would suggest

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focusing mainly on the most relevant of the research. For instance, the relationship observed between elongation and water depth is definitely not a strong one as stated in the manuscript and similarly the relationship observed for the edge density have a significant scattering. It is not clear to me what is the impact of these results in the economy of the manuscript. I suggest removing figure 3 and 4

Response: We agree that the paper could be made much clearer and focused by removing from the main text certain results that are less relevant to our central results. To that end we moved the results detailing patch-size scaling properties (i.e. patch-size vs patch-perimeter and patch-size vs patch-elongation observed in figures 5a and 5b) to the supplementary material, as these results require a somewhat complicated and technical explanation, and add little to the overall discussion. Specifically, this includes page 6 lines 1-10; page 11 lines 7-13; page 15 lines 14-21; page 20 lines 14 - page 21 lines 13; figure 5a and 5b of the original manuscript.

Likewise, we believe the manuscript is improved by moving results from maps M2 and M3 to the online supplement. These results largely serve to verify results of the primary map, M1, and require lengthy explanations, detailing caveats associated with mapping resolution impacts on our analyses. We found no significant disagreement between results from different maps. Specifically, this includes page 9 lines 18-24; page 15 lines 24-26; page 16 lines 1-12; plots corresponding to M2 and M3 in figures 1, 2, 3, and 4 of the original manuscript. This also allows us to combine figures 2, 3, and 4 into one composite figure (Figure 2 in the revised manuscript).

We feel that the relationships in figures 3 and 4 of the original manuscript (MWD vs elongation and MWD vs edge density) are important in explaining the hydrologic controls on pattern, specifically in regards to pattern degradation from hydrologic modification. These results point to a depth-dependent fragmentation process, as well as a loss of sloughs at very low water depths. We have edited the discussion to clarify and emphasize these implications (lines 371-377 and 378-382 of the attached draft), but have retained the results in the revised paper.

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Comment: 3) All regression must include the level of significance of the correlation.

Response: The manuscript has been edited to include specific significance values (see lines 283-297 of the attached draft)

Comment: 4) Figure 5 - line a: the authors observed a positive correlation (although very weak  $R=0.10-0.00$ ) of the patch elongation with patch size. From my point of view, this weak correlation may be influenced by sampling errors due to the difficulty to correctly measure the elongation at very small scale. Please include a comment on this.

Response: To account for error in measuring fine-scale features, we omitted features less than 100 m<sup>2</sup>, which was a conservative threshold given the high resolution of our primary map. We agree that observed correlations are weak, but we argue that they are compelling because similar behaviors are observed across a wide range of sites. Given the complexity of patch shapes, patch elongation is a less direct and intuitive measurement than others we employ (such as area or perimeter). To that end, we propose the addition of the following to the section on these results:

“The significance of these results is somewhat unclear however, as the behavior of patch-based elongation for complex, non-euclidean shapes is relatively unexplored. Correspondingly, we emphasize some caution in the interpretation of these results, particularly in light of the relatively weak correlations observed. Nonetheless, we find that the consistency of this result (i.e. patch-based elongation having a positive correlation with patch size) across a wide range of sites compelling, which may point to an intrinsic property of the landscape features.”

Comment: 5) The reference to Scanlon et al. (2007) is missing in the reference list.

Response: The reference has been added.

\*\*\*\*âĀĀ Response to anonymous referee #2

Comment: 1) The manuscript is well written, but is overly rich in very discipline-specific

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jargon that makes it difficult to read and interpret even for this reviewer who knows the Everglades system very well. I fear that the paper will be largely inaccessible for most readers in its current, highly technical format. There are some very important conclusions being made in this manuscript, albeit in very subtle and hidden ways. If the authors could find a way to repackage their findings in clear, easily understood conclusions, the paper would have much more impact.

Response: To address the issues posed by the reviewer, we have made numerous changes to the manuscript, particularly in the discussion, with a focus on readability and flow. We have moved two less relevant results which require a very technical explanation to supplementary materials, namely patch-elongation vs MWD and patch-perimeter vs MWD, allowing us to focus on the core results. Results from supplementary maps (M2 and M3) have also been moved to supplementary materials, as their inclusion in the main text required lengthy explanations of effects of different map resolutions on our results. The results from M2 and M3 were consistent with the primary map, M1, and add little to the discussion. We also moved the details of the distribution testing methods to the supplementary materials, as these are highly technical and not central to understanding the core results. By moving these sections to supplements, the core results of the paper are easier to interpret.

We have also rewritten several sections with a focus on replacing technical and discipline specific jargon. For instance, the section in the discussion regarding the distinction between a global or non-global limitation (page 22 lines 22-28 and page 23 lines 1-5 of the original manuscript) has been rewritten in easy to understand terms of how the effect is spread out across a landscape, in place of discussing it in the technical terms of the rates of diffusion and effective diffusive lengths (see lines 429-439 of the revised manuscript). Also, as the reviewer notes, some of the implications of our results were somewhat hidden within the discussion, and correspondingly we have expanded on these implications in the discussion (see lines 334-341 and 378-382 of the attached draft).

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Comment: 2) One more issue that may be more philosophical than can be addressed in this paper. This analysis and most others before it all assume that we can learn deep things and make broad conclusions about the Everglades landscape of 150 years ago (i.e. pre- drainage) based on analyzing places in the landscape today that we deem to be similar to that prior condition. This is an unrealistic assumption and it can lead to misinterpretation of the conclusions, which is particularly dangerous when those misinterpretations are being made by decision-makers. It is true that most environmental restoration is not that at all—we are not taking existing ecosystems back to their pre-impact conditions. Rehabilitation is a much better term for this. And this semantic problem is big in the Everglades, where I constantly worry that the public's perception of "restoration" is far from what the reality will be. But in analyses such as this, it is critical that the [obviously talented] scientists writing up their data make it VERY CLEAR that their analysis is relative to the current condition of the landscape, and has little or no bearing on the way the landscape originally formed a millenia ago. I encourage these authors to caveat their findings and conclusions with this in mind.

Response: We agree that making inferences about pre-drainage conditions based on current conditions can be problematic given the absence of pre-drainage imagery. We have explicitly noted this caveat in the discussion (lines 341-353). However, we also note there that some of the pre-drainage landscape features remain in the current landscape, albeit in a degraded state. Recent paleoecological evidence (Bernhardt and Willard, 2010) suggests that many large ridge features present in the pre-drainage landscape have remained relatively stationary, corresponding to features in the current landscape, supporting our view that 2-D geometry is not uniformly lost. More importantly, some properties are invariant over a wide range of contemporary hydrologic conditions (i.e. patch-size distributions and r-spectra), suggesting that these properties are likely robust to hydrologic change, and therefore representative of pre-drainage conditions. In contrast, properties that show significant variation across hydrologic condition (e.g. density, perimeter, elongation) likely have less resemblance to pre-drainage conditions, but are thus useful for interpreting how pattern adjusts to hydrologic changes.

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Correspondingly, we have added a section highlighting this important caveat and the associated implications.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 2975, 2015.

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