

Response to anonymous referee #1

Comments to the Author:

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 restoration actions? What is the impact of human activities on the observed spatial patterns?; What are the driving factors controlling specific configurations?

Response:

We agree with the reviewer and thus we have added a section that discusses the implications of our results (see lines 469-473 of the revised manuscript).

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 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 with the reviewer and we have substantially reduced the manuscript length, resulting in a much clearer and focused paper. We removed from the main text certain results that are less relevant to our central results. 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, however 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 moved the results from maps M2 and M3 to supplementary information. The results from M2 and M3 largely serve to verify the results of the primary map, M1, however require lengthy explanations in parts, detailing the caveats associated with mapping resolution on our analyses. We found no significant disagreement between these results and that of the primary map (nor any significant additional insights from these results), and correspondingly propose that they be moved to supplementary. 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 (see figure 2 of the revised manuscript).

We do however feel that the relationships observed in figures 3 and 4 of the manuscript (MWD vs elongation and MWD vs edge density) are important in explaining the hydrologic controls on pattern (specifically in regards to the degradation of pattern from human-induced hydrologic modification), pointing to a depth-driven fragmentation process, as well as a loss of pattern at

very low water depths. We have edited the discussion to clarify and emphasize these implications (lines 350-361 of the revised manuscript).

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 293-300 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:

In order to account for error in measuring fine scale-features, we omitted features that were less than 100 m², which we found to be a conservative threshold given the high resolution of our primary map. We agree with the reviewer that the correlations observed are weak, although we still find them nonetheless compelling given that they show similar behavior across a wide range of sites. We have added the following text to articulate these points:

“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.

Response to anonymous referee #2

Comment: 1) The manuscript is well written, but is overly rich in very discipline-specific 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: In order to address the issues posed by the reviewer, we have made numerous changes to the manuscript, particularly in the discussion section, with a focus on readability and flow. We have moved two less relevant results which require a very technical explanation to supplementary materials, as described above in response to Reviewer 1 comment 2. We have 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. We found that by moving these sections to supplementary, the core results of the paper are much 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 431-436 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 added a section that reviews and highlights such implications (see lines 469-473 of the revised manuscript).

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 with the reviewer that making inferences on pre-drainage conditions based on current conditions can be problematic given that records of the pre-drainage landscape are absent. However, we expect some semblance of the pre-drainage landscape features to be present in the current landscape, albeit in a degraded state. A recent paleoecological study

(Bernhardt and Willard, 2010) suggests that many large ridge features that were present in the pre-drainage landscape have remained relatively stationary, corresponding to features in the current landscape, supporting this view. Furthermore, properties that show invariance over a wide range of condition (i.e. patch-size distributions and r-spectra) suggest that these properties are fundamentally robust across a wide range of parameter space, and therefore may be more likely to be representative of pre-drainage conditions as well. However, properties that show significant variation across hydrologic condition (e.g. density, perimeter, elongation) have no bearing on pre-drainage conditions, but are nonetheless useful in interpreting hydrologic response to the extant pattern. Correspondingly, we have added a section highlighting this important caveat and the associated implications (see lines 397-406 of the revised manuscript).