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Interactive comment on "A microtopographic signature of life: Ecohydrologic feedbacks structure wetland microtopography" *by* J. S. Diamond et al.

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Anonymous Referee #1:

We thank Referee #1 for their detailed review of our manuscript. We have broken out your individual comments (RC) and responded to each accordingly (AC). We hope that our comments address and clarify any issues or concerns that they may have.

Overall comments:

RC1: I find the introduction to be quite lengthy though (see comments below). I also found that the introduction could be better organized to more clearly present the hy-

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potheses.

AC1: We will revise the introduction to be shortened and to more clearly present the hypotheses, leaving any mention of hypotheses until the last paragraph; see responses to comments below.

RC2: It seems to me the authors have mainly described the distribution (spatial and size) of microtopographic features and potential drivers of microtopographic features (e.g. water table depth, subsurface mineral soil depth), but have not, in my opinion, provided substantial evidence that these features lead to self-organization as suggested (L502-507). Particularly the feedbacks between plants and microtopography was not even studied, although it was mentioned frequently in the introduction and speculated about in the discussion. I believe the author's story would be much stronger, in regards to self-organization, if some attempt to quantify plant communities was made.

AC2: We agree with the reviewer that without some more context that the results presented are not in of themselves evidence of hummock self-organization. We will now discuss in text that this manuscript is coupled to a sister study (in review) that examined in detail the vegetation communities and soil chemistry of hummocks and hollows in these wetlands. That work further supports our hypothesis that hummocks are indeed self-organizing as a biotic response to inundated conditions. The depth and breadth of results from both studies are too much to present in one manuscript, thus the two separate manuscripts. We will emphasize this and that our overarching objective for the coupled studies was to explore patterning and its drivers. We also realize that the some of the diagnostics that we used to assess self-organization (i.e., nearest-neighbor distances, size distributions, bimodality) may be unfamiliar to some of the audience. However, these are commonly used in the literature as strong indicators of self-organization and feedbacks, so we have more effectively conveyed their usage as such in the Introduction and throughout the manuscript. Specifically, we will revise the introduction to more clearly establish how diagnostics from the field of landscape ecology can suggest patterning mechanisms but that measures of patterning should

be coupled with those of hypothesized drivers. We will then scope this paper's objective as focused on the former while referencing our sister study as one that focuses on the latter. Our overarching objective was to use both studies to explore microtopographic patterning and its drivers. We will also re-organize discussion text to first present our observations, emphasizing patterning and how it varies within and across systems as a function of water table position, and then present what the observed patterning suggests using well-established diagnostics from landscape ecology and when considering our complimentary observations (from the sister study) on vegetation and soil properties on hummocks vs. hollows.

RC3: It would be interesting to see a study that actually looked at formation of microtopographic features over time (maybe using a chronosequence).

AC3: We agree, but these features can take decades or more to form. Some work has been done by Benscoter et al. (2005) after fire in peat bogs, and some work also has been done using geochronology in salt marshes by Stribling et al. (2007). We have now added discussion about the difficulty in such studies, but that the few have done it have shown further support the general hypothesis that wetland hummocks are relatively stable, long-lived, and generated and sustained by plant action.

RC4: I suggest the authors acknowledge some of the limitations of the study in testing the self organizing hypothesis (primarily no plant-soil-microtopography feedbacks were measured, and changes in hummock hollow size/distribution etc. was not measured).

AC4: We will be sure to include more limitations of the inferences allowable by this study in the Discussion and Conclusion. We note again that we will also reference our companion study that does measure plant and soil properties of these systems.

RC5: I would like to see more reference to other forested wetlands, as I feel that was somewhat lacking.

AC5: We conducted extensive literature reviews as part of this work, and one of the

reasons the findings are novel is that this is one of the few instances of microtopography being measured in this way in forested wetlands. The primary systems that have focused on self-organizing microtopography have been Sphagnum moss bogs, fens, and marshes.

Detailed Comments:

Abstract:

RC6: I find the second half of the abstract to be quite vague with no data/percentages or any other time of numerical evidence for all the findings the authors "showed". I would suggest putting a little bit more information on the actual findings in the abstract so the reader has something to pull them in.

AC6: We will update the abstract to include more specific numerical findings as suggested.

RC7: Each of the last six sentences begin with "we. . ." Although I don't mind some use of personal pronouns, this seems excessive and detracts from the writing. I would suggest changing at least a few of the sentence structure to avoid this.

AC7: The authors acknowledge the comment and will reconsider the stylistic choices for the use of "we".

RC8: What is meant by "base elevation"?

AC8: We will clarify that "base elevation" refers to the average elevation of the hollow surface.

RC9: L27 What is meant by "reactive surfaces" of hummocks? Reactive with what? Does this mean biogeochemically reactive? Or reactive with the plants?

AC9: We will clarify that "reactive surfaces" refers to the effective soil surface for redox gradients and exchange interfaces. Because many of the coupled redox reactions (e.g., nitrification and denitrification) happen at soil-water interfaces (where there are

steep gradients in reactant concentrations), the expansion of the effective surface area enhances overall wetland biogeochemical reactivity.

RC10: L27-28 What is meant by specific yield in reference to surface water dynamics?

AC10: Specific yield is a common term in hydrology which can be (for wetlands) defined as the ratio of input (precipitation) or output (ET) depth relative to the induced water level rise or fall (e.g., if 1 inch of rain causes a 2 inch water level change, specific yield = 0.5). For the sake of brevity in the Abstract, we will most likely keep this term as is, but more clearly explain it in the Broader Implications section, where the term is introduced.

Introduction:

RC11: The introduction is too long and needs to flow better. Some of the paragraphs become quite redundant and could be greatly shortened or condensed. For example, the paragraph from L63-77 discuss positive and negative feedback loops on formation and maintenance of microtopographical features. L98-115 the authors again discuss feedback loops with very similar. It seems like this could be condensed into one paragraph or two smaller paragraphs (one for positive feedbacks and one for negative feedbacks, or one paragraph for both).

AC11: We will make efforts to condense and simplify the explanation presented in the introduction as it is clearly a source of confusion. We note that there are two types of feedbacks discussed in the two paragraphs: (L63-77) refers to horizontal organization of microtopography and how to diagnose patch dynamics, and (L98-115) refers to vertical organization of microtopography. Still, we will clarify and simplify these points, which will hopefully help convince readers that our methodology for diagnosing self-organization is based on precedent and substantial theory.

RC12: L86 “which are frequently modelled with powerlaw functions”. I am not sure what this adds to this paragraph, if anything I found it to be a disruption to the flow of

the paragraph. L89-97 typically hypotheses are presented in the final paragraph. In this case the authors present a hypothesis, and then go onto numerous more paragraphs describing feedbacks in the formation of microtopography. I would suggest moving hypotheses to the final paragraph after all justification for the hypotheses has been given previously.

AC12: We understand the frustration with the flow of the introductory narrative, and we will work to simplify and clarify. Briefly, we will note that introducing modeling of size distributions for hummock patches provides insight into the feedback processes that maintain their sizes: power law distributions indicate system-scale negative feedbacks to hummock expansion, and exponential distributions indicate hummock-scale negative feedbacks to hummock expansion. Further, as mentioned in a previous and later response, we will hold presentation of hypotheses for the end of the introduction.

RC13: L121 “unsaturated soils compared to unsaturated soils”...change one of the unsaturated to saturated

AC13: Thank you for finding this error; we have corrected it.

RC14: L129-135 I am not sure what this paragraph adds to the introduction. Do the authors plan to test the null hypothesis? If so, why not just move/incorporate that first sentence into the concluding paragraph (with other hypotheses, as already suggested)? I would also suggest stating all the hypothesis similarly. Either state all as null or state as the alternate, I prefer the latter.

AC14: We do test this hypothesis specifically (hypothesis 2), line 145. Either way, this is clearly confusing and we will work to clarify the Introduction. We will now reserve all mention of hypotheses for the end of the introduction.

RC15: L153 “regularly spatial patterned” awkward wording.

AC15: Acknowledged, but this is a common term in the self-organization literature, so we have elected to keep it as written.

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RC16: L136-165 Here the authors do provide a detailed list of their hypotheses, which is what I would expect. But mixed in with all the other hypotheses the authors present in the introduction (scattered throughout) it is confusing as the reader to know which hypotheses are being tested and which are not. I think all mention of hypotheses should be removed from the introduction, other than the final paragraph. The authors should streamline the introduction to better guide the reader through the main arguments they are making that set the stage for the hypotheses in the final paragraph.

AC15: All of the hypotheses listed here are the ones we explicitly tested. We will clarify the rest of the Introduction so there is no confusion on this matter. We think that this upfront clarification may help with all of the rest of the comments throughout this review, as well.

Methods:

RC16: L175 I don't think ET was previously defined. . ..it is actually defined on L190.

AC16: Thank you, we will fix this.

RC17: L319-320 I am confused by this sentence. The authors say there was a clear difference in resistance when a mineral soil layer was reached. So the authors are saying that the rod method is quite unreliable? Or are the authors just stating that this is why they took 2-3 measurements per area? Just want to clarify.

AC17: The rod method is very reliable, but we wanted to acknowledge that occasionally we would hit some buried wood or a tree root (which was obvious by feel). In these rare instances we would sample depth in the near vicinity to avoid the root and to ensure we were reaching a mineral layer. We will remove this sentence because it just adds confusion and is not important to the results.

Results:

RC18: L421-422 I don't see a figure showing seasonal water table depth

AC18: Table 2 contains the hydrology metrics for each site. We do not have a figure for seasonal water depth because it is not a result that we thought contributed to the overall manuscript. We can provide a supplementary figure of water table time series if the editors deem it necessary, but all relevant hydrology metrics used are presented in Table 2.

RC19: L423-433 It sure would be nice if the authors had real data on hummock plant species taxonomy, biomass, etc. It would strengthen the manuscript significantly and really help support some of their conjectures on plant feedbacks with microtopography. Do the authors have any data on this? If not, is this section necessary, as it is observational and not technically data driven?

AC19: We do have this data in a companion study that we will now reference and summarize findings in the discussion.

RC20: L460 Why “possibly”? It either is or isn’t. Looks to me that L2 is definitely not and D1 is not if you are considering alpha of P<0.05 as significant. It is if you are using P<0.1 as signiïňĄcant. I actually am not sure I saw any mention of that in the methods section.

AC20: We agree with the reviewer have deleted the word “possibly” and replaced it with “except for D1 and L2”.

RC21: L471 Add “7” to “(Figure)”. It would be helpful to also identify here that the authors (I think) are referring to the top panel of Figure 7.

AC21: Thank you for finding this omission; we have clarified as requested.

RC22: L487 Add “8” to “(Figure)”.

AC22: Thank you for finding this omission; we have clarified as requested.

Discussion:

RC23: L520 which ïňĄgure?

AC23: This should be Figure 6, thank you for finding this omission.

RC24: L524 what ecosystem/wetland type is Watts et al. 2014? In forested wetlands, there may be much more propensity for formation of microtopographic features, particularly because trees typically root more heavily in the elevated/aerated hummocks which likely further raises the elevation of these features. I wonder how that would differ in a different wetland type.

AC24: The wetland in Watts et al. 2014 is the ridge-slough mosaic of the Everglades, FL, and have now added this clarifying information. We agree with the reviewer that it seems reasonable that there may be more propensity for formation of microtopographic features in forested wetlands, but most research on this topic has occurred in non-forested wetlands, so there are still open questions in this regard. We suggest that a major difference between microtopography in forested versus non-forested wetland systems will be the size distributions and spacing of hummocks. In forested systems, hummocks associated with trees will likely be limited in size, exhibiting characteristic sizes and spacing due to local negative feedbacks from the crown competition. In contrast, non-forested wetland hummocks may have a much wider distribution of size classes. Negative feedbacks to non-forested hummock expansion may range from local nutrient competition effects (e.g., Eppinga et al. 2008), or system-scale inundation effects, where hummock presence increases overall water levels through displacement (Heffernan et al. 2013).

RC25: L525 What other surface level processes?

AC25: We will be more explicit that we are referring to soil production processes that result from hypothesized feedbacks between increased organic matter production (and therefore increased soil height) on hummocks.

RC26: L531-534 how would you measure what "state" a hummock is in? This seems like it would be the ideal experiment to test your self organizing hypothesis, ie to test over time (or using some well thought out chronosequence of sites) the forma-

tion/change in microtopography (or repeat this study in x amount of years).

AC26: We agree with the referee that a time series experiment with and without disturbance would be ideal to test this hypothesis and we will add text to suggest this idea for future work. Hummock state could be measured through time series of production (potentially measured with hummock volume and hummock soil bulk density) and respiration (potentially measured with chamber methods). When production exceeds respiration, the hummock is in a "growing" state, and when production equals respiration, the hummock is in an "equilibrium" state.

RC27: L613 here and other places, seem to lack some of the more current publications in forested wetlands on microtopography. For instance, researchers out of John King's lab group at North Carolina State University have many numerous publications from forested wetlands in coastal NC on hummock-hollow microtopographical distribution and also effects of this on soil and plant processes. Not sure if this work would help but there seems to be only comparisons in the manuscript to northern, non forested wetlands. It would be nice to see some inclusion of more relevant literature cited.

AC27: We have now included the following recommended publications in our discussion of forested wetlands, each of which supports our conceptual model that although local organic matter production is higher on hummocks, leading to increased hummock height/volume/area, greater respiration due to unsaturated conditions eventually balances these increases:

Minick, K. J., Mitra, B., Li, X., Noormets, A., & King, J. (2019). Water table drawdown alters soil and microbial carbon pool size and isotope composition in coastal freshwater forested wetlands. Frontiers in Forests and Global Change, 2, 7. This work provides observational support for our contention that hummocks are loci for increased soil organic matter and soil organic matter processing (respiration) relative to hollows, a finding which is also common to non-forested wetland systems.

Miao, G., Noormets, A., Domec, J. C., Trettin, C. C., McNulty, S. G., Sun, G., & King, J.

S. (2013). The effect of water table fluctuation on soil respiration in a lower coastal plain forested wetland in the southeastern US. Journal of Geophysical Research: Biogeosciences, 118(4), 1748-1762. This work similarly supports the notion that respiration is higher on drier hummock microsites compared to wetter hollows, which is a key feature of our conceptual model.

Miao, G., Noormets, A., Domec, J. C., Fuentes, M., Trettin, C. C., Sun, G., ... & King, J. S. (2017). Hydrology and microtopography control carbon dynamics in wetlands: Implications in partitioning ecosystem respiration in a coastal plain forested wetland. Agricultural and forest meteorology, 247, 343-355.

Minick, K. J., Kelley, A. M., Miao, G., Li, X., Noormets, A., Mitra, B., & King, J. S. (2019). Microtopography Alters Hydrology, Phenol Oxidase Activity and Nutrient Availability in Organic Soils of a Coastal Freshwater Forested Wetland. Wetlands, 39(2), 263-273.

RC28: L629 what does EAB stand for?

AC28: Emerald ash borer, an invasive beetle that causes extreme ash tree mortality. We will add this information.

RC29: L623-631 I am thinking that some of this information would best to put more upfront in the discussion, and maybe in the introduction. The fact that the authors did not measure (or present) any vegetation data but rely heavily on their interpretation of microtopographical features is somewhat problematic. Therefore, it is important to alleviate the readers concern that there was no need to measure this. Can the authors comment on why no vegetation measurements were taken in the current study?

AC29: Yes, as noted throughout, we will reference the companion study.

RC30: L633-L638 Seems like the concluding section is too short. I think it could just be wrapped into the previous section on Broader Implications or expounded upon to make the conclusions a little more impactful.

AC30: Noted, we will consider these options in our rewrite of the Discussion.

Tables and Figures:

RC31: Figure 1. Add space before Incipient in caption. What is meant by soil mass? Is this specifically referring to the amount of soil or just to the soil as a whole? Also, I wonder if arrows between the incipient events are needed. For instance, tree fall could of course be caused during extreme weather events.

AC31: We will add the space. Soil mass refers to the amount of soil, which we will clarify in the caption.

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