

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

Anonymous Referee #1

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The authors of this manuscript use a laser scanning data set to characterize the spatial distribution/occurrence of hummock and hollow microtopography in forested wetlands of northern Minnesota and evaluate whether feedbacks between hydrologic and soil factors are influence of the formation and maintenance of microtopography within forested wetlands. This manuscript provides a detailed description of microtopographic features within different landscape types within these wetlands (Depressions, lowland, and transitional areas). Overall the writing is very good. 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 hypotheses. On a picky note, I found that the lack of line numbers for every line was a nuisance when trying to make specific

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comments/suggestions (maybe this is the journals request). I am unable to comment specifically on the TLS method to analyze hummock and hollow patterns, because I am not directly familiar with this type of analysis. 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. It would be interesting to see a study that actually looked at formation of microtopographic features over time (maybe using a chronosequence). The authors provide evidence that surface microtopography cannot alone explain surface microtopography and that hummock elevation is positively correlated with water table depth. I do agree with the authors (L517), that the relationship between organic matter depth and subsurface mineral soil suggest that more buildup of organic matter is happening on previously elevated hummocks, thereby supporting the maintenance of that hummock. 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). I do think this manuscript provides a very nice description of microtopographical features in forested wetlands and inclusion of hydrological and soil drivers adds nicely to current knowledge of wetland microtopography, and therefore is of interest to readers/scientists within the field. I would like to see more reference to other forested wetlands, as I feel that was somewhat lacking. Detailed Comments: Abstract: 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. Each of the

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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. L21 What is meant by “base elevation”? L27 What is meant by “reactive surfaces” of hummocks? Reactive with what? Does this mean biogeochemically reactive? Or reactive with the plants? L27-28 What is meant by specific yield in reference to surface water dynamics? Introduction: 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. Then, on 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). L86 “which are frequently modelled with power-law 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. L121 “unsaturated soils compared to unsaturated soils” . . .change one of the unsaturated to saturated 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. L153 “regularly spatial patterned” awkward wording 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,

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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 Methods: L175 I don’t think ET was previously defined. . .it is actually defined on L190. . . 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. . . Results: L421-422 I don’t see a figure showing seasonal water table depth 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? 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 significant. I actually am not sure I saw any mention of that in the methods section. 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. L487 Add “8” to “(Figure)”. Discussion: L520 which figure? 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. L525 What other surface level processes? 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 formation/change in microtopography (or repeat this study in x amount of years). 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

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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. L629 what does EAB stand for? 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? 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. Tables and Figures: 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.

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