

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

Anonymous Referee #2

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SUMMARY: The authors have composed a wonderfully in-depth study of hummocks in *Fraxinus nigra* forests in Minnesota. Hummocks are a feature that is often noted and quickly characterized in the literature, but I would agree with the authors that this is the most detailed descriptive work I've seen on forested wetland hummock structure and the study incorporates an interesting laser-based approach to measurement. I find the research to be technically sound, but some of the narrative of the paper goes beyond what is directly supported and sort of distracts from the good descriptive work that the authors have done. ___ For example, one big finding here is that degree of hummock formation is a function of inundation. (The authors' "Lowland" sites being less-developed with regard to hummocks due to intermittent inundation shows this re-

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ally well). That kind of simple finding is basic but noteworthy in my opinion. I'd support the authors developing that point more and drawing in similar findings from the literature. Instead the authors often seem to veer into discussions of causal mechanisms for such patterns, and that's where I'd say they go beyond what the data collected can really support and what can really be claimed. ___ That leads to some slightly overstated conclusions, such as on Line 634. I agree that structure and pattern were well described in a new and interesting way. However, I disagree with the idea that drivers of wetland microtopography were detailed in this study; some leads were generated here (hummocks are not probably not a mere addition to subsoil microtopography and it seems to be all about hydrologic regime. and the authors are clearly aware of the many candidate parameters that could affect hummock formation... they simply were not studied in detail within this particular paper). ___ I think the Intro and discussions of this paper need to be steered back toward the descriptive results at hand rather than the often vegetation-based mechanisms that *might* be at work. I tried to suggest a few changes that would help that shift in my comments below.

COMMENTS: ___ The title is too broad and emphasizes the part of this work that is less known (i.e., the mechanistic nature of feedbacks that maintain hummocks); and this is about hummocks in *Fraxinus nigra* forests, as opposed to all other types of wetland microtopography so more specificity is warranted in the title. ___ The third sentence of the abstract introduces a complex vegetation-centric hypothesis that the current study seems unable to really address or resolve. This idea of testing microsite preference is an example of something that I would bring up in the discussion as a possible next venue for research, but raising it in the abstract seems out of place since it is certainly not at the center of this research effort ___ very broad start to the introduction, I think the first paragraph or two could be shortened. ___ The paragraph beginning on line 53 seems to set up two non-exclusive scenarios. plants find their preferred microsite on existing substrates or plants create hummocks. Can't it be a little bit of both? Showing that hummocks are self-organizing doesn't change the fact that various species may be "hummock specialists" once the pattern is set. ___ I like the development of

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positive and negative feedbacks idea around lines 70-77, this is great as context for the patterning, even tho I don't think the present approach really allows us to discern what mechanisms are at play.... in the following paragraph tho the authors go on to say that previous authors have argued that overdispersion of patches can be taken as evidence of negative feedbacks (which I think makes sense). If others have already made the case well, the authors should dig in and say a little bit more about how this connection of pattern and mechanism has been argued previously. (connect the dots a little bit more for the reader about how these "inferences" are made.) ___ Lines 116 and 117 of the introduction cite a figure (which is fairly uncommon in intros). I happen to like the model, but I think it should be used differently. At present, the authors seem to work in this order: 1) propose a model based on previous work in the literature early in the intro, 2) state more specific/basic hypotheses about that patterns they are likely to see (e.g., taller hummocks in more inundated sites), 3) present findings and discuss. . . . I suggest doing part 2 in the intro, part 3 thereafter, and weaving part 1 into the discussion (maybe put the figure into supplemental material but reference it); that way the paper moves efficiently through the topography-centric research and ends with some broader (untested) ideas about what's really going on via specific mechanisms involving plants/soils/water on the ground. ___ I found the hummock area calculation curious (Line 271). Was this just a best-guess method by the authors? when they say that their method provided a conservative estimate of height are they comparing that to a field-based measurement with an autolevel or a meter stick or something? (obviously this might be a best-guess scenario, but I'm just curious) ___ The contention that hummocks plot above the -1:1 line in all sites (in manuscript lines 466 and 467) does not seem to be supported by sites L1 and L3. (as far as I can see in Figure 6). Result may need to be stated differently there. (it's also problematic in view of the fact that approximately one and a half sites lacked data b/c the horizon was below detection with your depth to refusal rod method. I think the authors did the correct thing by omitting those data, but it further weakens that claim that "all sites" showed this trend.) ___ Lines 545-560 are great, but this is where I would suggest the authors actually spec-

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ulate more about what drives hummock formation and what is the same or different about hummock formation in forrested systems and others. For example, the authors cite work by Lawrence and Zedler 2011, which showed that inundation drove tussock formation and correlated with tussock height (just like the present study); those authors also showed that the tussocks they studied were majority organic (so I wouldn't lump them in with "soil building" as stated in Line 585). I think the authors ought to capitalize on an opportunity to compare and contrast more... drawing out with what is same/different from the hummock literature vs. their results. ___ Line 567 and 568 makes a claim that "this study is the first..." I'm not so sure that's the case. I recall a paper by Bruland and Richardson in 2005 (not cited here) that looked at hummock and hollows in natural wetlands as a natural counterpart to similar features a restoration site study. And more broadly, the authors might want to check the most recent Foundations of Restoration book chapter by Bruland and Zedler (because it's a review chapter of wetland microtopography) as a way of seeing if they truly are the "first"... ___ I think the authors should reconsider what they present in paper vs. in supplement. The star example of this is Figure S2. As a first-time reader of this paper, I'm most eager to see what the impressively data-rich TLS approach turned up and to see what the hummock pattern looks like!!! I want to see Figure S2... I would include that one (and possibly Figure S1) in the paper itself, even if it means shunting other tables and figures into the supplementary materials; (something like Fig 9 is extremely cool to be able to draw, but to me it's far less important to the main theme of the paper).

SMALL COMMENTS AND CORRECTIONS: ___ Eppinga et al. 2008 is first referenced on page 2, and several times after that, but there is only one Eppinga ref in the ref list dated as 2009. Please double check the citations here as it's unclear if the intent is to cite a single paper or two. ___ In line 96, it would be useful to say more than "meaningful structure." Is there a more specific signature that the authors would assert represents autogenic feedbacks at work? I'm not sure why the burrowing, litter accumulation, and erosion would preclude regular spacing (overdispersion) of hummocks. ___ On line 102 I see a reference to a familiar citation (Barry et al. 1996)

about hummock formation in forested wetlands, but the citation does not appear in the ref list. ___ eliminate "just" in line 130 (redundant). ___ I like the explanation of HGM categories (Line 180-185), very helpful. ___ Results section 3.5 is really neat. I see it as a strength of this paper that the lowland sites (with their less inundated hydrologic regime) showed a different (essentially less hummocky) topography. very cool! ___ Line 545 says "(Figure);" A figure number is needed there. ___ Line 560 has an extra comma in the last citation. ___ In the Figure 3 caption, refer to each individual photo by its caption (e.g. D2) and remind the reader what D, L, and T denote.

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