Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-239-AC3, 2019

Hydrology and Earth System Sciences
Discussions
Open Access
EGU

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

J. S. Diamond et al.

jake.diamond@irstea.fr

Received and published: 10 September 2019

Anonymous Referee #2:

We thank Referee #2 for their detailed review of our manuscript. We have broken out your individual comments and responded to each accordingly. We hope that our comments address and clarify any issues or concerns that they may have.

Overall comments:

RC1: 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

Printer-friendly version

Discussion paper

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 really 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.

AC1: We appreciate that without additional context, the conclusions and mechanisms discussed in this paper may seem to reach beyond the results presented. We will do a better job in the Introduction and throughout to justify our inferences and rationale for causal mechanisms. First, we will now mention 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. We also realize that 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 landscape ecology literature as strong indicators of self-organization and feedbacks, so we will be sure to more effectively convey their usage as such in the Introduction and throughout the manuscript. We further believe that addressing the causal mechanisms is what makes this work relevant and interesting because these mechanisms are present in most wetlands, and therefore the invoked feedback loops are applicable in most wetlands to understand vegetation-soil-water feedbacks. Last, we agree that we could emphasize our finding of less microtopographic structure in drier sites; indeed, this finding is consistent with our proposed causal mechanisms.

Specifically, we will revise the introduction to more clearly establish how diagnostics from the field of landscape ecology can suggest patterning mechanisms but that mea-

Printer-friendly version

Discussion paper

sures 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. (The extent of results from both papers precluded presentation in one paper.) 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.

RC2: 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).

AC2: We will temper our conclusions to more accurately reflect what was specifically found in this work (i.e., by focusing on specific patterning and patch metrics). In our edits to the Introduction and Discussion, we will also more strongly present that in this work we aimed to infer process from pattern, using site differences in hydrology as a test of our hypothesized driver for microtopography. We note again that this work has a companion study (in review) that more thoroughly addresses drivers and consequences of wetland microtopography in these systems, the results of which are bolstered by analogous findings in non-forested wetlands.

RC3: 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

Printer-friendly version

Discussion paper

comments below.

AC3: We appreciate the Referee's intention here, and we will reorganize and edit the Introduction and Discussion to better prepare the reader for the results presented and the mechanisms invoked; see AC1.

RC4: 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.

AC4: We have changed the title to: "Pattern and structure of microtopography implies autogenic origins in forested wetlands". While we still believe that our results are largely consistent with the original title, we have revised it to be more tempered ('implies') and specific to forested wetlands . However, we do not think that just because our study focuses on one type of ecosystem (as the vast majority of studies are) that it is limited in scope of inference. Indeed, the mechanisms invoked are consistent with microtopography studies in wetlands throughout the globe and our approach adds to these studies by using a theoretical landscape ecology perspective to further evince the self-organizing properties of wetland microtopography in forested wetlands.

Abstract:

RC3: 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.

AC3: We agree with the reviewer that this sentence is out of place with this work and we will focus the abstract on microtopography structure and patterning. We will save this type of language for future work in the discussion.

Introduction:

RC4: Very broad start to the introduction, I think the first paragraph or two could be shortened.

AC4: Acknowledged; we will rewrite this section to be more clear as to the intent of the paper.

RC5: 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.

AC5: Agreed, and we try to be mindful of this through our language in this paragraph (e.g., the words "the degree" in "wetland vegetation simply preferentially occupies hummocks (sensu Jackson & Caldwell 1996) versus the degree to which wetland vegetation reinforces and maintains its own hummock microtopography"). We do not mean to suggest that it is one or the other, but only that through examining spatial patterns of hummocks that we can make inferences about the mechanisms that lead to their persistence throughout time.

RC6: I like the development of 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.).

AC6: We will work to make our application of these ideas and previously established diagnostic tests are more clear for the reader throughout the Manuscript, but especially in this part of the Introduction.

RC7: 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.

AC7: We have considered the reviewer's suggestion for reorganization, but opted to keep the current organization as is. We believe that with planned substantial edits to the Introduction and Discussion based on the constructive comments in this review that the text will now flow more naturally. We submit that the current organization most accurately aligns with the history of this work. The hypotheses and conceptual model came from initial observations of microtopography in our model systems (black ash wetlands), but also from literature and previous experience in other wetlands. We then sought to test this conceptual model using a novel approach using concepts from landscape ecology and patterned landscapes. In other words, the conceptual model is intricately linked to, and in fact informs, our hypotheses. Again, we hope that with the planned edits that this will be clearer in the revised work.

Methods:

RC8: 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 conser-

vative estimate of height are they comparing that to a field-based measurment with an autolevel or a meter stick or something? (obviously this might be a best-guess scenario, but I'm just curious).

AC8: We agree and have now changed the text and side area calculation to be based on a conical shape (a reasonable estimate for hummock shape), where we estimate lateral area from measured volumes and heights. Updated calculations are approximately 50% larger than the initial 20-percentile height, which was a best-guess conservative estimate to calculate hummock side area. We chose this initially rather than, say, using the median or average height of the hummock because hummocks are not perfect cylinders (they are more tapered at the top). Hence, we were trying to provide a lower bound for what we expect the additional hummock side area is. We believe the new approach (cone shape assumption) to be conservative based on our observations of hummock shapes, which tend to be have more undulating edges (as opposed to simple conical sides) that would lead to further increases in area above the conical estimation.

Results:

RC9: 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.)

AC9: We thank the reviewer for the careful attention to detail here. We appreciate the reviewer's point, and we will add text to refine our presentation of results by noting that not every measured point exhibited this trend. We note here that at sites L1 and L3, only 1 hummock plots below the 1:1 line (at approximately -0.5m mineral layer elevation at site L1), so we believe our overall statement is well supported. Moreover, that

the drier lowland (L) sites had less clear patterns in this regard than our considerably wetter depression (D) or transition (T) sites supports our hydrology-driven hypothesis for hummock development. We believe that because some of the points were too deep to reach with our rod (particularly at site T1) more likely provides further evidence for our contention that hummocks are self-organized mounds on a smooth surface of organic soil, rather than an argument against it. If we had a longer rod, the chances that a point would plot below the -1:1 line at these highly organic sites are exceedingly small because the buildup of organic matter to a thickness of greater than 1.2m (the rod length) will almost certainly be smoothed out by physical processes like flooding and wind over long time-scales. We think that this is an important point that could have been better presented; we will revise text to emphasize what these deep (non-data) points suggest.

Discussion:

RC10: Lines 545-560 are great, but this is where I would suggest the authors actually speculate more about what drives hummock formation and what is the same or different about hummock formation in forested 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 558). 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.

AC10: We will edit this section to more clearly draw similarities and distinctions between our study and others. We will also clarify that we included organic matter as part of conceptualization of "soil building" here because hummocks in our system are also primarily organic matter.

RC11: 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".

AC11: The authors have conducted extensive literature reviews as part of this work and are familiar with both the Bruland and Richardson 2005 study and the Bruland and Zedler book chapter, and acknowledge that our study is not the first to examine microtopography in forested wetlands. Our language is precise regarding what we studied (i.e., regular patterning and hydrologic control), and we believe it is accurate to the best of our knowledge: "but to our knowledge, this study is the first to demonstrate regular patterning in forested wetland microtopography and the hydrologic control on this regular pattern emergence." To avoid any issues, however, we have revised the text to temper the language: "Regular patterning of landscape elements is observed across climates, regions, and ecosystems (Rietkerk and van de Koppel 2008), and here we demonstrate such patterning for forested wetland microtopography and, importantly, demonstrate the hydrologic controls on its patterning and structure."

RC13: 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).

AC13: We also share the reviewer's excitement regarding the TLS approach and its applicability. The initial presentation of these results and the TLS methodology was presented in previous paper, Stovall et al. (2019; DOI: 10.1016/j.rse.2019.111271),

which is why we did not add it initially. We will now add a portion of them to Figure 3 to create a 6 panel figure showing both photos and TLS results. We note Figure 9 is a critical finding that supports our hypotheses by inferring process from pattern. We acknowledge that we have not done a good job of highlighting this, and will make edits accordingly to support the inclusion of Figure 9 as a critical result.

Specific comments:

RC14: 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.

AC14: Thank you for finding this error, we have corrected these references.

RC15: 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.

AC15: We will be more specific with this phrase in our edits. There are three signatures that represent autogenic feedbacks: one vertical (bimodality), and two lateral (overdispersion of patches and characteristic patch sizes); we test each of these in this work. Regular spacing induced by random processes would be extremely rare, and the probability of this is testable using metrics and methods which we discuss later in the Introduction and Methods.

RC16: 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.

AC16: Thank you for finding this omission, we will add this to the references.

RC17: Eliminate "just" in line 130 (redundant).

AC17: We have deleted this word.

RC18: I like the explanation of HGM categories (Line 180-185), very helpful.

AC18: Thank you!

RC19: Results section 3.5 is really neat. I see it as a strength of this paper that the low-land sites (with their less inundated hydrologic regime) showed a different (essentially less hummocky) topography. very cool!

AC20: We agree, and we suggest that this finding (hydrologic control on microtopographic structure) has similar support with all of our results.

RC21: Line 545 says "(Figure);" A figure number is needed there.

AC21: Noted, have fixed this; should be Figure 9.

RC22: Line 560 has an extra comma in the last citation.

AC22: Noted, we will delete this comma.

RC23: 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.

AC23: Noted, we have added these recommendations and also updated the figure to include Figure S2 as additional panels.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-239, 2019.

Printer-friendly version

Discussion paper