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Discussions

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

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Response to Short Comment

Victor Lieffers: We thank Dr. Lieffers for his 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:

SC1: I am, however, not convinced that this work shows 'that the structure and regular patterning of wetland microtopography is an autogenic response to hydrology.' On

(L635-36). There are a variety of external influences such as frost/ice, severe disturbance in drought, floods, wind that could be influencing these wetlands.

AC1: We acknowledge that other external influences may influence microtopography in these systems. Black ash wetlands are a relatively understudied ecosystem relative to other wetlands, but we believe we can discount each of the listed influences as being more important than hydrology for the maintenance and persistence of hummock hollow microtopography. Frost and ice: it is possible that frost preferentially affects soil expansion and contraction in these systems thereby leading to regular horizontal and vertical patterning, but this mechanism would have to persist throughout the growing season and among years. We expect that the organic nature of these soils and the regular inundation in the growing season would likely erase any frost-based microtopographic signature on a yearly basis. In contrast, the observed persistence of microtopographic features in our system and in numerous other documented wetlands more strongly point to a balance between increased soil organic matter production from vegetation on local high points that is eventually balanced by increased soil organic matter respiration due to aerobic conditions. This mechanism would persist throughout years, and would unlikely be erased due to frost upheaval. Drought: we are unsure through what mechanism that drought may induce microtopography in wetlands, but of course, we are open to alternative hypotheses. Floods: while our wetlands experience regular inundation throughout frost-free season, they rarely experience high-energy flood events that could reorganize soil structure. Wind: black ash wetlands are susceptible to tree fall by extreme wind events, which can create microtopography (indeed, as initiators in our conceptual model, Figure 1). However, we suggest that the patterns of tree fall alone are highly unlikely to explain our observation of regular spatial patterning (i.e., common spacing between hummocks, and a characteristic hummock size). A less complex hypothesis, and one that we believe is supported by our multiple lines of evidence, and by previous studies in wetland systems, is that hydrology (the primary physical control in wetlands) is a major driver of microtopographic structure through our proposed feedbacks.

SC2: Some of the basic questions posed in this study seem rather simplistic and most folks who have worked in peatland systems would already know this. Indeed figures 5, 6 and 7 would be predicted by simple logic before collecting and analyzing such data. As a consequence, I rate the novelty of this work as rather low. I suggest that the authors refocus their data on the detailed spatial arrangement of hummock and hollows and stay away from this autogenic feedback idea – because frankly, I am unconvinced from what is presented.

AC2: The authors strongly disagree that the results presented could have been easily ascertained a priori. We are unsure what simple logic the commenter is referring to with regards to Figures 5, 6, and 7. In this work, we uniquely extended concepts from landscape ecology to assess microtopographic patterning and potential feedbacks, a novel application. Hence, our unique contribution is the observations of patterned signatures that are reflective of coupled feedbacks. We systematically test these signatures with commonly applied diagnostics by evaluating elevation bimodality, characteristic patch spacing (hummock nearest neighbor distances), and truncated hummock size distributions. We have made edits in the Introduction and throughout to more clearly point out that underlying processes can be inferred through spatial pattern analysis. Figure 5, shows elevation bimodality at our sites (with the strongest bimodality occurring in our wettest sites and the least bimodality occurring in our driest sites). We did not know a priori that 1) we would observe any bimodality at our sites, or 2) that hydrology would be as strong a predictor for the differences in bimodality and hummock area among sites. Bimodality on its own is evidence of autogenic feedbacks that create and maintain hummock structure, as has been illustrated in analogous non-forested systems, but is by no means a commonly measured or reported finding in the literature. We also reiterate that our finding of elevation bimodality being limited to our wettest sites further indicates that hydrology is a major driver of topographic divergence into hummocks and hollows. Figure 6 is a direct test of whether surface microtopography represents subsurface mineral microtopography or whether it is decoupled from subsurface structure (thereby indicating it is generated by surface processes, supporting our hypotheses),

but we did not know whether this would be true before we tested it. We also did not know what the distribution of organic matter depths were in these wetlands because this is invisible to the eye and inherently non-obvious. Figure 7 provides direct support for hummock height being a function of local hydrologic conditions, both at the system and at the microsite scale. If this were simple logic, according to the commenter, then it would also be simple logic that hydrology drives hummock structure, thereby negating the first comment presented indicating that the results were unconvincing in this regard. We however agree with the original commenter's first comment that there could be many reasons for wetland microtopographic structure, but we hypothesized that it was driven by hydrology and we directly tested that hypothesis here. We are currently making revisions to our text to more clearly present our evidence, which we contend supports the autogenic hypothesis of hummock development.

SC3: I would have expected some more information on the types of vegetation on the tops and sides of hummocks vs the sides and bottoms of hollows.

AC3: We are now referencing a companion text (in review) that directly measures vegetation communities and soil chemistry on microsites throughout our study systems. In that work, we find further support for the hypotheses presented here (i.e., that hummocks are maintained by vegetation through an organic matter production-respiration feedback that depends on hydrology).

SC4: L141 Prediction 1 that elevation distribution will be bimodal in a hummock hollow system seems to be a rather mundane prediction as you selected the study site with such characteristics.

AC4: We did not select study sites with elevation bimodality a priori; in fact, we would argue that three of our sites exhibited very weak bimodality. We were working in these systems as part of a separate, larger work studying black ash systems in detail due to their vulnerability to loss from the emerald ash borer (Diamond et al. 2018, Slesak et al. 2014, Looney et al. 2015). In the course of our work, we observed the hummock-hollow microtopography of these systems, which we selected for further study. We are also unsure how one would know if a system had bimodal elevation without actually measuring it.

SC5: L145 Prediction 2. This would only be relevant with shallow peat. There are plenty of studies that show that peatlands often spread across the landscape over thousands of years of peat accumulation so there is very little reason to think depth is that important, after a minimum depth of peat is achieved.

AC5: What is the minimum peat depth that the commenter thinks is relevant to our hypothesis? We, of course, could not know ahead of time what the peat depth of these systems were. For instance, peat depths varied by over 1m across our sites, and although all sites had some degree of organic soil, not all sites had actual peat; some were more mucky mineral, and some were more sapric. By no means are all black ash wetlands considered peatlands, but that we observed more microtopography in sites with the most organic soils (which corresponded to wettest sites) is further evidence for our proposed feedback to the vertical structure of microtopography. (Plant production of organic matter preferentially builds up around plant stems/roots builds, but this production is ultimately constrained in height by aerobic soil respiration when the hummock becomes too dry).

SC6: L153. It would be good to have a better understanding of the negative process that maintains a hollow part of the landscape. If there is not a powerful process that tears these substrates apart, what will maintain the hollows over the decades?

AC6: We have clearly not done an adequate job of explaining the hypothesized feedbacks that maintain hummock-hollow structure in wetlands, and will improve this in the upcoming revisions. We do not believe there is a “powerful process that tears [hollow] substrates apart”. In our conceptual model, hollows are areas of mean/base elevation within the wetland, whereas hummocks are local high points raised above this mean/base elevation. Hence, hollow maintenance is essentially just lack of “invasion”

by hummocks, which themselves cannot grow to infinite size (as shown in Figure 8 and 9) due to negative feedbacks to their expansion (e.g., crown competition for light).

SC7: L181-190 I did not really understand the description of the hydrology of these sites. These are quite generic descriptions of these landforms. It is would be nice to know more about the freeze-thaw cycle of the peat and how this might be a factor in hummock hollow distribution and the types of disturbances that might periodically affect these wetlands. What is the frequency of extreme flooding and drought in these systems? Could flooding be a primary reason for maintenance of patterns and hence be linked to wetness? What is the lateral flow of water through the peatlands and does this have any effect on the physical movement of the hummocks during times of extreme flood? Further, later in the paper there is a statement that hummocks are often associated with an ash stem. Perhaps this is an important mechanism that should have been explored in your spatial study.

AC7: We will add some information about freeze-thaw cycles in the upcoming revision, but note that black ash wetlands are understudied with respect to most other northern peatlands, so much remains to be discovered. We would be open to a mechanistic hypothesis that links flooding to observed patterns (hummocks in these systems do not physically move to the best of our knowledge), but we consider our conceptual feedback model to be appropriate and supported by evidence presented here, in the companion paper in review that we will now reference, and in other low-energy wetland systems. Lateral flow through these wetlands (not peatlands) is primarily groundwater, and other related work indicates that this is a small fraction of the total water budget compared to precipitation and evapotranspiration. The fact that hummocks are associated with an ash stem is actually the primary rationale behind our proposed feedback model and subsequent hypotheses, and we reference this throughout the manuscript. We will be more explicit in revisions to address this issue.

SC8: L195-205 What is the density of trees? Canopy cover or leaf area index of the forest? I would like to know what sort of substrate this forest is growing on. Is it

Sphagnum peat or Carex or feather moss? Paper like this needs some discussion of the mosses/herbs and graminoids that cover the ground surface in such places.

AC8: This data is presented in our companion paper, but we will include some basic information here for reference in the Methods section.

SC9: L319 Using a steel rod and resistance to pushing down - how was buried wood distinguished from mineral soil at the base of the peat? If wood was encountered, then averaging with 3 other nearby spots will produce an underestimate of the true depth. Incidentally, it is traditional to use a spoon-like end on such probes to bring up a bit of mineral soil to confirm that it has been reached.

AC9: Buried wood was clearly distinguishable from mineral soil by touch and when it was encountered, the rod could easily penetrate further in the nearby vicinity to reach the mineral layer. We are removing this sentence from the next revision because it overcomplicates the issue. . .buried wood was rarely encountered in our fieldwork.

SC10: L325-30 You did not really tell us where the well was positioned relative to hummock hollow system. What was the control height to which you were defining the water level? I could not figure this out from the datum description.

AC10: We placed the well at "the approximate geographic center of each site" (Line 329–330). "We referenced each site to a datum located at each site's base well elevation." (Line 227). So, the well was essentially in the middle of the site and we referenced all elevation to the base of the well. We will try to be more clear in revisions.

SC11: L339 What is the elevation of the well and what is ground surface (mineral soil or top of hummock)?

AC11: The elevation of the well is our datum, so 0 m, and ground surface is the boundary between the soil and the atmosphere.

SC12: L141 The hydroperiod is based upon 1 year (I think?). Given the dynamics of moisture from one year to the next, is this long enough? Also was this a typical year. I

expect that very wet years might influence the dynamics of these stems for years after.

AC12: The hydroperiod is based on 3 years of data (2016–2018); we will be more explicit here.

SC13: Fig. 5. Please remind us what the O elevation means.

AC13: We will clarify in the figure caption.

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