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Discussions

Interactive comment on "Modelling the habitat preference of two key *Sphagnum* species in a poor fen as controlled by capitulum water retention" *by* Jinnan Gong et al.

Anonymous Referee #3

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General Comments In this manuscript, the authors develop and validate a simulation model (Peatland Moss Simulator, PMS) that combines an underlying (and previously published) peatland hydrology model with individual-based Sphagnum surface physiology and community components (unpublished) to be used in future studies projecting peatland response to environmental change. They suggest that the PMS will better capture the "feedback between the plant community structure and the environment" that is lacking in other models. In my opinion, this is a very worthwhile approach at the present as there is much known about each of the components, and their degree of variation, that they incorporate into their model; the field is ripe for such a dynamic, quantitative summary of community and ecosystem processes in Sphagnum domi-

nated peatlands.

Overall, this is a well-written and well organized manuscript. The main modeling approach is clearly laid out in Fig 1 and the components well described and justified for each. My expertise lies in the area of physiology/ecology and I read those sections with particular interest. The relationships between capitulum water and carbon dynamics were thoughtfully approached. Indeed, their ability to highlight the importance of water balance parameters relative to ones that affect growth is an important result (L507-525). Also, I appreciated their using a carbon allocation model that segregated non-structure carbon (NSC) as the pool for new growth. This level of physiological mechanism of often left out. In general, I found the level of their modeling suitably mechanistic. This also applied to the competition model where an individual approach tied to growth and competitive interactions seems appropriate, especially given the well-developed state of knowledge in this area.

I do like the modeling within each of the surface components. However, the strength of the manuscript is linking them to an underlying hydrology model, which provides a sound ecological context. In addition, their tests against predictions and field data seem appropriate.

That said, in my opinion, there are areas that could be improved. The major and minor ones I list below with less important ones indicated in the Specific Comments section. Major Comments A. The Abstract and Introduction focus on feedbacks between the plant community structure and the environment. It seems from the outline of the model (Fig 1) and the descriptions of it that the environment serves as more of a forcing variable on the plant physiology and community dynamics. For example, there are no processes that feedback to the "Community environment" module in their model (Fig 1) and I did not see any not listed within the descriptions of the model structure in the text. Clearly there are feedbacks between the capitula environment module and the shoot growth and competition module, but I don't think the capitula environment is really what people would consider part of the plant's environment. Fixing this will

reframe the justification, but I think it can still be well justified. B. In my opinion, the paper would be improved by applying the model to make predictions about a particular response to an environmental change. It could be argued that this paper is for model development and validation and the next one will use it in a predictive context. However, is there a small question that could be addressed with the model that would illustrate its value? C. I was surprised that the model did not deal with any of the autogenic processes that lead to hummock formation. The community model is spatially explicit and it would seem that it would allow for rule-based hummock formation simulation when succeeding from a high water table. Instead, the model simulates either high or low water tables. This seems like hummock forming processes would represent a true feedback to the environment. Is this either desirable or possible in this model iteration? D. The living tissue of Sphagnum species clearly differ in their hydraulic conductivity (Km, p8; as shown in the McCarter and Price 2014 paper cited, see also Li, Glime and Liao 1992, J Bryology 17:59); however, this was treated as a constant. Although I do not think there are reports of how this differs between S. magellanicum and S. fallax, I think it would be important to consider variation in this using hummock and hollow values for the two. I suspect that this would only accentuate the differences they observe in their results, and/or, speed up the time until species distributions equilibrate. In any case, given that species cover changes are quite sensitive to Km (Table 3), I think it is worth modeling species-specific differences in this parameter.

Minor Comments E. I was surprised that the Titus and Wagner (1984, Ecology 65:1765) paper was not cited. Some of the simulation modeling is similar and should make for a nice comparison. F. Need a table of abbreviations. G. It would be very helpful to show how the parameter values used fall within reported ranges in the literature (e.g., Table 1).

Specific Comments 1. Line 81-2 Aren't they linked by capitulum water balance? Retention is too specific, I think. 2. L101-4 I find this sentence confusing. Can you be more clear about the linkages? 3. L142-3 I think it is controlled by water contentâĂŤnot

the same thing as water retention. 4. Fig 1. What is the difference between dashed and solid lines? Can the boxes or arrows be changed so it is easier to tell that Module III influences Module IIâĂŤit took a while to realize it wasn't controlled by precip and evap, where I thought the arrows were coming from. I would suggest making the figure legend more complete. 5. L213-18 This is the discussion of reseeding. It would be useful to know how frequently this was necessary. Was it rare with little impact on results or more common? 6. L380-82 Is it worth listing what the parameters are meant here in text as is done below? 7. Fig 2. The y-axis for the top figure should be "Relative Cover". Also, can you use solid and dashed lines to distinguish Hummock from Lawn? Would make it easier to read on B&W print. 8. L415 Why not show both species in both environments? Here only show S. mag in hummocks and S. fal in lawns. 9. L418-23 Would it be better to report these as elasticities (% change in outcome per % change in parameter). This is easy to do as they were all set to vary by +/- 10%. However, this would allow you to assess whether or not it was a large change or notâĂŤwhat would cutoff be? You report that being less than the standard deviation for a 10% change is meaningful (L490), can you defend that? 10. L469 You state that S. fallax capitula were less resistant to evaporation, but the data in Table B1 seem to indicate otherwise (see ra; this result is opposite to what I would expect although the do not differ significantly). 11. L492 Yes, it would be expected for n to have a large effect as it is a scaling factor, so changes in its magnitude get amplified. 12. L502-06 See Comment D above. 13. L968 The procedure for doing the photosynthetic measurements would seem to cause quite a lot of drying within the cuvette (RH 60%, impeller at level 5) where they were measured over 60-120 minutes. Were they rewetted following each light level? Were they allowed to dry? How did mass change during the course of the measurement and did that influence shape of curve? Can you provide a light response curve showing data? If there are not good answers to these questions, it would at least be helpful to include how the parameters measured compare with other ones in the literature.

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