Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-20-RC1, 2019

Interactive comment on "Assessing the peatland hummock-hollow classification framework using high-resolution elevation models: Implications for appropriate complexity ecosystem modelling" *by* Paul A. Moore et al.

Anonymous Referee #1

Received and published: 2 April 2019

General comments

Moore et al. present a methodology to classify the peatland hummock-hollow variability for carbon flux modeling using high-resolution elevation models with k-means clustering. The study has collected samples across a variety of sites and mapped the high-resolution microtopography with the structure-from-model. This manuscript provides insights into the influence of microtopography on the uncertainties of field sampling and carbon flux modeling. Considering the importance of the peatland carbon fluxes to the global climate change, this study is relevant and necessary. Overall, this

manuscript is well-written and easy to follow. However, several issues in the manuscript need to be improved before the publication. This manuscript has done a nice analysis of the DEMs and evaluated its impacts on the NPP simulation. However, the validation of the generated DEM and the model-based fluxes is weak and needs to be strengthened. Another key issue for small scale flux simulation is to identify the optimal spatial resolution for modeling. I would also suggest the authors improve the analysis to identify the optimal spatial resolution to represent the microtopography. Generally, I think that this manuscript is publishable after revisions.

Specific comments

1. L20: Some key words are repeating from the title. Normally key words should be different from words in the title, as to provide additional information.

2. Abstract: The findings on the optimal spatial resolution is quite important for the appropriate complexity of flux modeling. As mentioned in L438-439, this manuscript concluded that on the optimal resolution to represent the spatial variability has been identified from Fig. 2 and 5. These findings should be reflected in the Abstract.

3. Validation of the generated elevation model is needed. The structure-from-motion technique is sensitive to the camera geometric calibration, camera position information, and the accuracy and numbers of ground control points. Validation results on the generated DEM are necessary.

4. L237: Equations should be marked with a number. For the equation at L237, the variable x should be explained.

5. L438-439: From Figure 5, it is not as easy as Figure 2 to identify the optimal resolution to represent the spatial variability. The authors can use additionally spatial analysis, e.g. semivariogram analysis, to strength these findings.

6. The structure-from-motion can provide both DEMs and orthophotos for the study site. In the manuscript, the authors have used the DEMs for data analysis. Potentially,

the orthophotos can be utilized to calculate vegetation indices to infer the vegetation growth conditions. This can improve the classification and NPP modeling. Why don't you make the best use of your data?

7. Table 1: Mistakes on the Longitude. For instance, Sweden should not be -17W. I guess the fourth column should be the elevation above the mean sea level instead of the longitude.

8. Table 2: In Table 2, some plots have been classified into three members. However, we cannot see such three members in the histogram distribution of Figure 3 and 4. Please explain the reason.

9. Figure S4. The curve should be from modeling and the dots are the measured one.

10. Figure S8: the scale bar should be added into the spatial map. Otherwise, readers don't know the spatial scale of these maps.

11. The paper has done a nice analysis of the carbon flux modeling and assessed the impact of water table depth on the carbon fluxes. However, it is hard to evaluate whether these modeling is accurate enough or not. It would be better to add some chamber or eddy covariance measurements to validate the simulated NPP. Or at least compare the results with other relevant studies.

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