Biogeosciences Discuss., 12, C4506–C4508, 2015
www.biogeosciences-discuss.net/12/C4506/2015/

Biogeosciences
Discussions
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Interactive comment on "Modeling micro-topographic controls on boreal peatland hydrology and methane fluxes" *by* F. Cresto Aleina et al.

Anonymous Referee #2

Received and published: 21 August 2015

This study develops a microtopographic model (Hummuck-Hollow) of the hydrology of a Russian elevated bog. This bog is an example of peatlands that are important components of the Earth system whose biogeochemistry are still poorly understood, especially regarding the modeling of processes that depend on fine-scale heterogeneity like methane emissions. Here, the authors incorporate a mechanistic treatment of the hydrology based on Darcy's Law for belowground flows and Manning's flow for surface water exchanges. The elevation on a horizontal scale as fine as 1 m is resolved, and the methane model is run concurrently at each fine-scale gridcell.

This study represents an important advance in the representation of these ecosystems, as it is the first to simulate 2-dimensional microtopography with realistic hydrology and

the inclusion of a methane biogeochemistry submodel. The authors show that the recession of the water table from the bog cannot be appropriately modeled without considering the microtopography, as the surface runoff is impeded by the alternation of hummocks with hollows, as compared with a control "bucket" bog model where the water is allowed to continuously run off. The study's qualitative results are very important. The result of the slower hydrological drainage is compelling.

The methane biogeochemistry is given slightly less attention than the hydrology, and the conclusions are also less novel for the methane biogeochemistry. The specific biogeochemistry of this particular site is not heavily utilized, with a very generic methane model and a crude NPP provided from an ESM. The basic conclusion is that heterogeneity in the water table depth results in a low bias for CH4 if only modeling the average depth; this has been shown elsewhere and follows directly from the nonlinear dependence of the water-table depth. Consequently, while I recommend this paper for publication with a few minor revisions for clarity, I do wonder if the more compelling hydrological focus of the paper may make it more appropriate for the Hydrology and Earth System Sciences or Geoscentific Model Development journals than Biogeosciences. However, the work shown here definitely has implications for biogeochemical cycling, so this decision should lie with the discretion of the editor.

Minor Comments:

1. Equation 1: Please explain in the Methods how this equation was chosen. The justification of this distribution as compared with others does not come until the results.

2. P. 10203, L. 6. Please explain how the Manning roughness coefficients for the hummocks and hollows were chosen.

3. Eq. 7-9 are difficult to follow and a citation would be appropriate.

4. P. 10204, L. 8. Please briefly explain how the methane model was tuned (currently this appears in the Appendix).

5. Is there any correlation between the bog depth, elevation, and landscape position? Does this affect the choice of simulating the elevation distribution as spatially-independent random samples? Following on with this question, would the conclusion about slowed surface flow be sensitive to the spatial pattern of the hummocks and hollows? For instance, what if they were patterned instead of random?

6. P. 10204. There is no discussion here (or in Section 4) about the litter chemistry and plant physiological controls that differences in vegetation exert on methane fluxes in hummocks and hollows. Here, it is implied that the main reason for increased emissions from hollows is a shallower water table, but this may also be due to the preponderance of sedges rather than sphagnum in some ecosystems (is this the case for this ecosystem?).

7. Figure 5 generally qualitatively shows higher emissions in hollows than hummocks. This provides moderate evidence that the hydrological controls alone (as opposed to the chemical controls mentioned previously that are not modeled) could explain the difference, although the scatter in the data is large. This moderate evidence should be noted in the Summary and Conclusions section and compared with other studies contrasting methane emissions from hummocks and hollows.

8. Figure 7 is excellent.

9. I am confused about the weak dependence on NPP shown in Figure 8. Is it showing that the NPP can vary over a large factor with little effect on the net flux? It seems like this is a crucial input for realistic fluxes in the methane model used (not important for the qualitative results of the study, as the NPP is treated as a constant for the whole bog). This figure is generally somewhat unclear and could be better labeled.

Interactive comment on Biogeosciences Discuss., 12, 10195, 2015.