Biogeosciences Discuss., 12, C5529–C5533, 2015
www.biogeosciences-discuss.net/12/C5529/2015/

Biogeosciences
Discussions
Open Access

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

F. Cresto Aleina et al.

fabio.cresto-aleina@mpimet.mpg.de

Received and published: 18 September 2015

We thank the Reviewer for the comments. We expanded our discussion section according to the Reviewer's suggestions, and we included his comments that helped a lot in and in clarifying the text of the revised version of the paper. We also included a deeper discussion of the limitations of the model following the Reviewer's 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.

We modified the text in the revised version, including a brief explanation of why we chose this equation.

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

We chose the parameters following:

Phillips, Jeff V., and Saeid Tadayon. *Selection of Manning's roughness coefficient for natural and constructed vegetated and non-vegetated channels, and vegetation maintenance plan guidelines for vegetated channels in Central Arizona.* US Department of the Interior, US Geological Survey, 2006. In absence of specific values for hummocks and hollows, we chose the values for light and medium to dense shrubs. We also tested the performances of the model for different coefficients in the range of 0.01 to 0.1. The output of the model did not qualitatively change our results.

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

We included the reference:

R. Manning. *On the flow of water in open channels and pipes.* Transactions of the Institution of Civil Engineers of Ireland, 161 – 207, 1891.

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

We expanded the description of the methane model tuning in the Methods section in the revised version of the text, including the information appearing in the Appendix. Now we introduce the concept of the tuning parameter R_0 already in the Methods section.

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?

We modified the micro-topography configuration using a more regular pattern (half of the peatland constituted by hummocks, and half of the peatland constituted by hollows). Our results did not qualitatively change for this test. We tried to simulate the micro-topography at the Ust-Pojeg mire complex, and therefore we chose this particular random configuration. Investigating this very interesting question more deeply was beyond the scope of the paper and it constitutes a new research question per se. In particular, future applications of the HH model to a peatland with a more regularly patterned micro-topography, like the one described by Eppinga et al. (2008), can potentially estimate the dependence of the micro-topography controls we described in the present paper on different micro-topography configurations. We included this observation in the Summary and Conclusions section.

In the Appendix, we included a sensitivity test for the case of no slope, to test the robustness of our results in case of a flat peatland with decreased lateral fluxes.

6. 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?).

This is true, and we included this observation in the discussion as a limitation of our model. The ecosystem is actually predominantly covered by *sphagnum* species, but the not modeled chemical controls could have introduced a bias in our results. The moderate evidence of the water table controls mentioned by the Reviewer in his following comment makes us confident that this potential bias is small.

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.

We included a more throughly discussion of limitations of our approach, and of the moderate evidence the Reviewer mentioned in this comment in the Result section. Following the Reviewer's suggestion, we discussed . We did not mention this evidence in the Summary and Conclusions section, since the focus of the paper is on model performances, rather than the discussion of experimental data. In the Summary and Conclusions section we included a wider discussion on the implications and the limitations of the particular methane emission model we coupled to the HH model.

8. Figure 7 is excellent.

We are happy that you liked it.

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.

The process-based model we used for simulating the methane emissions have a weak dependence on NPP, because the authors used the tuning parameter R_0 for different regions as a function of annual mean temperature and total annual NPP. Therefore, some of the dependence of the model to the NPP is represented by the specific choice of R_0. We showed in the figure that the potential bias we introduced by considering NPP simulated for C3 grasses and not the one of mosses is negligible. We introduced the concept of the tuning parameter R_0 in the Methods section in the text, and we included this information on the tuning parameter effects in the Appendix. We clarified the figure label, inserting the information on the effects of the tuning parameter on NPP.

References:

Eppinga, M., Rietkerk, M., Borren, W., Lapshina, E., Bleuten, W., and Wassen, M.: *Regular Surface Patterning of Peatlands: Confronting Theory with Field Data*, Ecosystems, 11, 520– 536, doi:10.1007/s10021-008-9138-z, http://dx.doi.org/10.1007/s10021-008-9138-z, 2008.

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