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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.

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We thank the Reviewer for the useful comments that helped a lot to improve the manuscript. We included the reviewer suggestions in the new version of the paper, also expanding our bibliography.

1. P 10197, line 7: To these two references, you can add a third WETCHIMP study that specifically examined model performance in high-latitude peatlands such as the one you are modeling: Bohn et al. (2015).

We included the reference in the revised version of the paper.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

2. P 10197, line 10: This is not quite true. The models LPJ-WHyMe (Wania et al., 2010) and UW-VIC (Bohn et al., 2013) had hummock and hollow components.

We clarified the text, acknowledging previous efforts in modeling hummocks and hollows.

3. P 10197, line 15-16: This is not quite true. In addition to Baird et al. (2009), Bohn et al. (2007), Bohn and Lettenmaier (2010), and Bohn et al. (2013) specifically examined the effects of water table heterogeneity on methane emissions (not just water table, as you imply on lines 17-18) from high-latitude peatlands, albeit restricted to West Siberia. While the first two papers used a TOPMODEL approach, Bohn et al. (2013) considered hummocks and hollows. In addition, Bohn et al. (2013) considered the impoundment of surface water (although not via the rigorous method you employed), which raised the water table in hummocks and hollows. What is new/novel in your approach is that you are handling lateral flow through the peatland in a process-based way (Bohn et al 2013 treated all hummocks and hollows as identical to a single hummock and hollow, without considering lateral differences and flow between them, therefore their approach was not completely process-based). Your approach is therefore much more realistic than any previous approach that I am aware of. So, while I agree that your approach is novel, I ask that you clarify how your approach is novel and different from the previous approaches.

We thank the Reviewer for the comments. We included these clarifications in the revised text, as well as the references to the papers cited in the comments. We now better underline that the HH model is not the first attempt to model water table heterogeneity influence on methane emissions.

4. P 10200, lines 3-27: There are two types of elevation represented here, microtopographic elevation Hi,j (described in the first paragraph) that you use to determine soil properties, local water table depths, and flow geometry, and a larger-scale elevation sli,j (described in the second paragraph) that you use to determine the slope that lateral

BGD
12, C5520–C5527, 2015

Interactive Comment

flow is sensitive to. But at this point in the manuscript, when you begin defining these two types of elevation, it is not clear how they are related. If I understand correctly, the cell's total elevation (what an altimeter would tell us) is the sum (Hi,j + sli,j), i.e. Hi,j is relative to some regional average elevation in the local neighborhood (which is sli,j). If so, then wouldn't it also be more correct to call this sum (Hi,j + sli,j) the "absolute elevation" and to call sli,j something more like a "macrotopographic" elevation? If I've misunderstood, please forgive me. Nevertheless, some clarification would be helpful. Please insert a brief explanation before the first paragraph (i.e. between lines 2 and 3) explaining the relationship between Hi,j and sli,j.

The Reviewer did not misunderstand, we included this clarification to the revised version of the paper, distinguishing as the Reviewer suggests between micro-topographic elevation and macro-topographic elevation. We also introduced the concept of "absolute elevation" following the Reviewer's suggestion.

5. P 10201, line 10: is Wi,j relative to sli,j, Hi,j, (Hi,j + sli,j) or sea level (or something else)?

$W_{i,j}$ is relative to the surface level, we included this information in the model description.

6. P 10203, line 6-7: typo, $\Delta Sl_{i,j}^{1/2}$ *is the square root of the slope, not the slope itself; also, shouldn't the "s" in Sl be lowercase, to be consistent with its definition in equation (2)?*

Yes, it is. We corrected this information and the typo in the revised version of the paper.

7. P 10215, lines 6-9: Your explanation here makes sense. However, it implies that the single bucket simulation accounted for the domed shape of the peatland (lateral flows, etc) despite consisting only of a single cell. How did you accomplish this, given that the lateral flows depend on the gradient in sl between neighboring cells? Did you compute lateral flows via analytical integration of the flow equation over the entire peatland (all

BGD

12, C5520–C5527, 2015

Interactive Comment

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Interactive Discussion

Discussion Paper

contained within a single cell) or did you use a value of N Âż 1 for computing sli,j, but set N=1 for computing Hi,j? Could you please insert a description of how lateral flows were handled in the single bucket simulation into the appropriate place in the Methods section?

We used indeed $N = 1$ for the Single Bucket configuration. We inserted this information in the text of the revised version, and we expanded the sensitivity analysis by changing the slope to 0 (please see comment below).

8. Related to the previous point: If you did not vary sli,j in the single bucket simulation (i.e., the single bucket simulation is completely flat and at the same elevation as the surrounding non-peatland), I would recommend doing so, as part of another control simulation. Or, if you did account for variation in sli,j in the single bucket simulation (i.e. the peatland was domed), I would recommend doing another control simulation with a perfectly flat peatland (not domed) with no lateral flow (or, perhaps, instantaneous flow? Not sure which would be most appropriate for the control simulation). Only by having both of these types of control simulations would you be able to separate out the effects of lateral flow (due to sli,j) vs. the effects of microtopography. Or did I misunderstand?

We did not include this control simulation in the first place because we tried to simulate the bog in the Ust-Pojeg mire complex, and therefore we tried to use parameters and sloe as close as possible to local conditions to compare the model output with .observations. It is true though, as the Reviewer observes, that our results could be influenced by the particular choice of the slope. Therefore we included in the appendix a new sensitivity analysis to this parameter, repeating the simulations for the case of zero slope. The lateral flux and the water table decreases are less pronounced than in the Standard Configuration case, but the general robustness of our results is confirmed. In the figures we see how, despite the zero slope, the HH model in the *Single Bucket* configuration still produces a water table lower than the HH model in the *Microtopography* configuration. The differences in methane emissions from the two model configurations are less pronounced, due to a more similar water table dynamics, but

BGD

12, C5520–C5527, 2015

the HH model in the *Single Bucket* configuration still misses the large peaks produced by the *Microtopography* configuration.

9. Could you comment on whether/how methane model parameters might affect the critical grid cell resolution? I can imagine that the rates of oxidation vs production, and the vertical profile of labile carbon, would affect the sensitivity of CH4 emissions to water table depth, and, in turn, the value of critical grid resolution.

In our analysis we maintained the vertical profile unaltered, as well as the rates of oxidation/production. It is true that the introduction of a change in those parameters could affect the methane response to water table changes. We included this information as a limitation of our approach in the discussion of the sensitivity analysis, as well as the lack of representation of litter chemistry.

10. Could you comment on how applicable this approach is to large-scale/global modeling (perhaps insert a brief section into your results and discussion section to discuss this)? Can this model be applied or easily adapted to permafrost conditions as well, or would the presence of ice lenses and/or limited active layer depth invalidate the approach? Would the computation necessary to compute a distribution of 106 cells per km2 be prohibitive at a global scale, or could some simplification/approximation be developed (a la topmodel with some sort of modification to account for the effect of microtopography)? How representative is the microtopography at the Ust-Pojeg mire of high-latitude wetlands – is it only representative of ombrotrophic bogs, or can it be applied to blanket bogs, patterned ridge-hollow complexes, fens, etc? I think many of your readers will wonder if they can use your approach in their applications – certainly I was wondering this – so a little bit of guidance could help convince others to use your approach.

We thank the Reviewer for the comment. We included a more detailed discussion of potential significance of this study for global modeling purposes. Answering the comment in detail, for the application to permafrost environments the model would

BGD

12, C5520–C5527, 2015

need some modification to take into account the influence of thaw depth on the lateral fluxes of water and on hydrology in general. The model is quite general in representing a typical boreal peatland and it could be easily adapted to both blanket bogs and fens, providing that the input and forcing data for the model are available. This study provides an evaluation of the HH model on one site, but we plan to test its performances also on other peatlands with different characteristics. The development of a parameterization based on the findings of the HH model which can account for the micro-topography effect at larger scales is under development and it is the subject of a follow-up paper that the authors submitted to Geoscientifical Model Development (now accepted for publication in Geosc. Model Dev. Discussions).

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

BGD

12, C5520–C5527, 2015

Interactive Comment

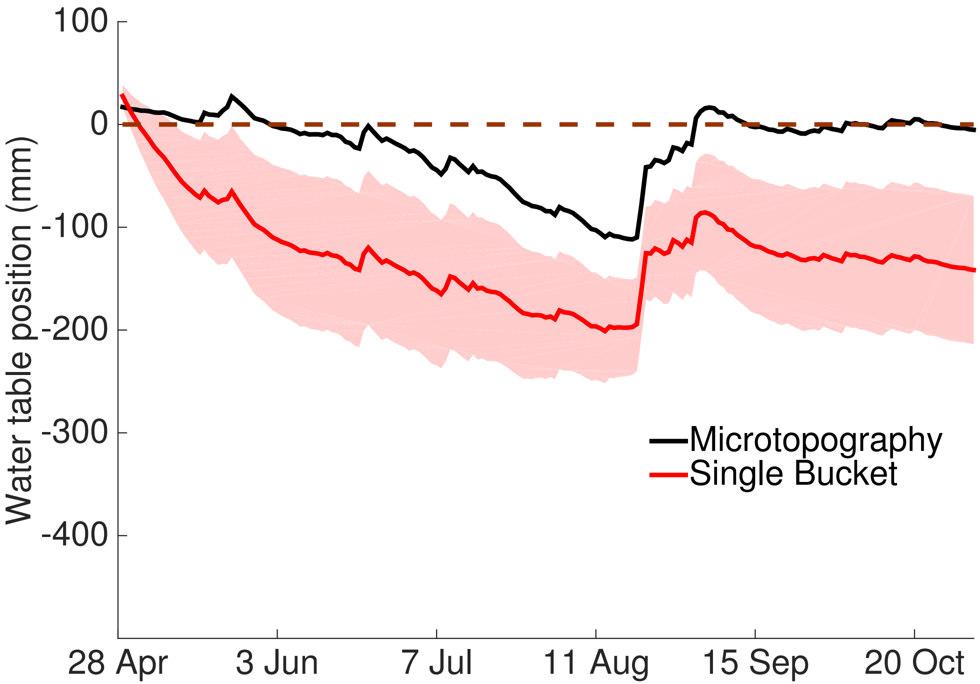

Fig. 1. Water table dynamics simulated by the HH model in the case of zero slope.

BGD
12, C5520–C5527, 2015

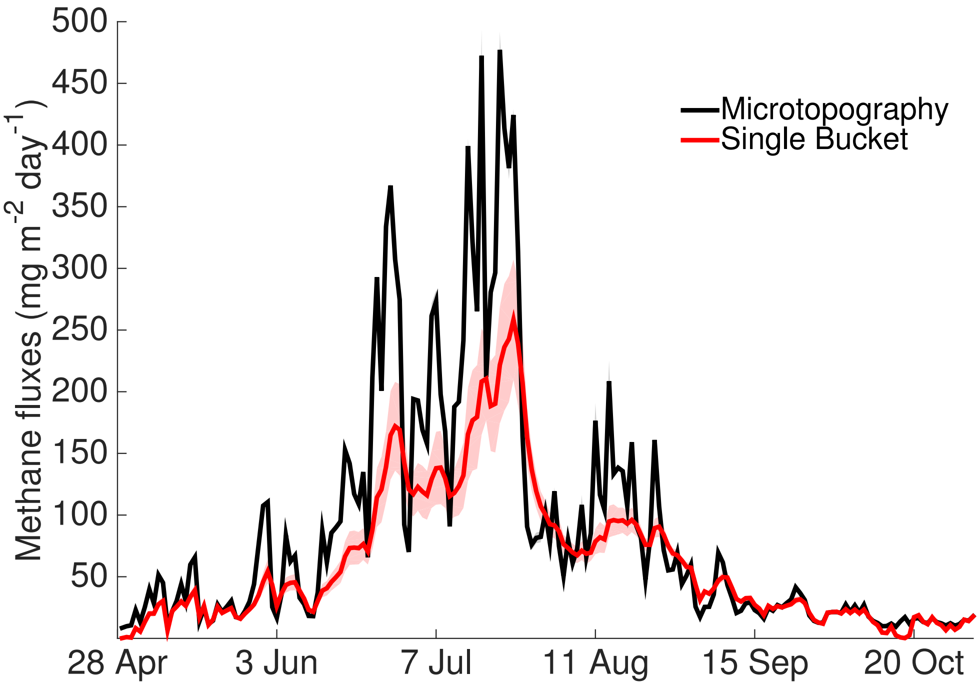

Fig. 2. Methane emissions produced by the HH model in the case of zero slope.

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12, C5520–C5527, 2015