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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 #1

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In this study, the authors present a process-based model of peatland hydrology and methane emissions that accounts for lateral above- and below-surface flows in response to both the larger-scale domed shape of the peatland and the effects of microtopography at the lateral scale of 1 m. The authors derive the dimensions and properties of the peatland from a combination of literature and in situ observations, and compare the modeled water table depths and methane emissions to in situ observations as well.

The study design is sound, and its presentation is clear and well-organized. The authors' approach is novel (although it is not the first attempt to model peatland microtopography and its effects on CH4 emissions, see below) and arguably more accurate than any previous large-scale models. This topic is both important and timely, since

there is both great concern and great uncertainty regarding the response of high-latitude peatlands to future warming. Some strong points of the paper include: the authors' use of intensive in situ measurements to both define the microtopography and to validate the model; the investigation of the effect of grid resolution on the results (to determine a minimum necessary resolution); the "single bucket" control simulation; and the sensitivity tests for NPP. I enjoyed reading this paper and think it will be a valuable contribution to wetland/methane modeling efforts.

I recommend publication after addressing the following comments:

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).

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.

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.

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 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.

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

6. P 10203, line 6-7: typo, ΔSli,j$\frac{1}{2}$ 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)?

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 contained within a single cell) or did you use a value of N » 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?

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?

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 CH_4 emissions to water table depth, and, in turn, the value of critical grid resolution.

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.

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