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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Topographic controls on soil moisture scaling properties in polygonal ground using idealized high-resolution surface–subsurface simulations" *by* G. Bisht and W. J. Riley

Anonymous Referee #3

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The authors present a very timely and useful method (actually two methods) for downscaling soil moisture variability in polygonal permafrost wetlands, and evaluate the methods by comparing to high-resolution modeling. It is timely because the possibility of widespread permafrost degradation in the coming 100 years requires improving our ability to model these systems accurately in global models and useful because this method would achieve performance similar to that of a fine-resolution model but can be used within relatively coarse-resolution earth system models that are used for projections of future climate change and impacts. I am impressed with how well this

method works and I think this shows great promise. A couple of things that I particularly like about this paper is that the authors investigated two alternative downscaling approaches, and also investigated the effects of vegetation heterogeneity.

Unfortunately, it appears that the deadline for comments has arrived 4 days earlier than my emails from HESS indicated, so I am not able to give as thorough a review as I had hoped.

I recommend publication with minor edits, which follow:

P 12837, lines 13-21: Soil moisture heterogeneity, manifested as water table heterogeneity, arising from microtopography of the type you are studying, has been shown to have an effect on carbon fluxes (specifically methane emissions) from high-latitude wetlands as well, in terms of both spatial mean methane emissions and their sensitivity to long-term climate change (Bohn et al., 2007, Bohn and Lettenmaier, 2010). P 12837, lines 22-29: I'm surprised you haven't mentioned the TOPMODEL approach (Beven and Kirkby, 1979), which produces a pdf of water table depths as a function of local topographic slope and upslope contributing area. This has been used in many modeling studies, from watershed to large scales. Several large-scale biogeochemical models have applied this approach to carbon fluxes (again, specifically methane) from high-latitude wetlands, with the idea that this might account for some of the influence of microtopography: Bohn et al. (2007), Ringeval et al. (2010), Bohn and Lettenmaier (2010). Another approach, in light of the recognition that water table depth may not be accurately modeled by TOPMODEL on the scales of microtopography, has been to derive the pdf of water table depths from assumed microtopographic elevation above a reference datum (Wania et al., 2010; Bohn et al., 2013). Again, while water table depth is not exactly the same as soil moisture, it can be related to soil moisture with various assumed moisture profiles (e.g., van Genuchten relationships like the ones you show) and it is very relevant to the study you are doing (high latitude wetlands). Yet another approach is the pdf of soil moisture produced in the Variable Infiltration Capacity (VIC) model (Liang et al., 1994) which derived its pdf formulation from the Xinanjiang model (Zhao et al., 1980). TOPMODEL and VIC have been used in hundreds or even thousands of studies. I think it's worth mentioning them.

I think the paper would benefit from a brief discussion of how this method can now be applied in global models. You used a LIDAR DEM here, but use of a LIDAR DEM across all of the high latitudes might be infeasible. Is there a way that global modelers could build on this approach, using the (coarser) global DEMs such as the ASTER DEM, perhaps taking advantage of self-similarity scaling relationships? And could this method be applied to non-permafrost peatlands?

Again, I am out of time and haven't been able to review this as thoroughly as I'd hoped. Hopefully my comments are helpful.

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