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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

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Summary:

The study "Topographic controls on soil moisture scaling properties in polygonal ground using idealized high-resolution surface–subsurface simulations" by G. Bisht and W.J. Riley investigates the impact of microtopography of the polygonal tundra on soil moisture distribution. The study is based on the hydrological model PFLOTRAN using a very high resolution DEM of the Barrow field site as topographic input. The hydrological model is forced by precipitation and ET rates obtained from a site simulation using the land surface model CLM4.5. In addition, PFLOTRAN is modified to include surface water flow. Simulations are performed with different DEM resolution in order to gain insights into scale dependencies. The results are used to infer downscaling procedures in order to calculate sub-grid soil moisture variability.

General comments:

This study concerns the very important aspect of soil moisture heterogeneity in tundra landscapes which largely controls biogeochemical processes such as the degradation of organic soil material. The study aims to provide approaches to downscale soil moisture variabilities which is also a very important aspect concerning earth system modeling. However, I have some major concerns about the used methods in this study:

I. According to the model description, the used version of PFLOTRAN seems to not include active layer dynamics which is one of the most essential processes controlling permafrost hydrology. In addition, the model seems to disregard spatial differences in the surface energy balance and other interactions between hydrology and heat exchange processes. The subsurface water flow in permafrost landscapes is largely controlled by the active layer dynamics. The active layer thickness controls (i) the subsurface water storage capacity and (ii) hydrological conductivity (size and shape of the aquifer). Polygonal tundra landscapes are usually characterized by very heterogeneous thaw depth evolution due strong spatial differences in the soil thermal properties and the surface energy balance (e.g. Langer et al., 2011, Boike et al., 2013). The rims of low center polygons typically feature lower thaw rates than polygonal centers. At the beginning of the summer period, the frozen rims form very efficient hydrological barriers which do not allow lateral water flow except for the drainage of precipitation and melt water from the rims to the polygonal centers and troughs. During the course of the summer period, spatial differences in the frost table equilibrate allowing lateral water exchange (Helbig et al., 2012). The gradual disappearance of the hydrological barriers strongly affects the soil moisture distribution which in turn has a strong feedback on the surface energy balance and soil thermal regime. These very basic interactions between active layer thickness and soil hydrology seems to be not included in the model. Therefore, it is questionable whether the performed simulations are able realistically represent the hydrological processes at a polygonal tundra site.

II. The study does not make any attempt to control whether the simulated soil moisture evolution is realistic. To the my knowledge, the Barrow site is one of the best instrumented field sites in the Arctic including several soil moisture and water table monitoring stations. Since the simulations of this study are performed with a DEM and the meteorological forcing of a specific site and not for a synthetic landscape, it is highly recommended to control the quality of the performed simulations. In particular, this is important due to the limitations of the model criticized above.

III. It is not clear why the model is extended to include surface water flow. Surface water flow in polygonal tundra occurs during very specific situations such as the very beginning of snow melt when the soil is still completely frozen or during flood events. However, there is no explanation on how surface water flow affects the performed simulations and why it was important to include.

IV. According to the model description, the distribution of soil moisture in PFLOTRAN seems to be determined by only one input variable which is the microtopography. Therefore, it is not surprising that the following analysis reveals a strong relation between soil moisture distribution and topographic indices. In addition, it is also quite obvious that reducing the resolution and thus the variability of the governing input variable results in reduced soil moisture variability. I think, the used method to analyses the spatial scales on which hydrological processes occur is appropriate. However, it is a prerequisite that the model includes all governing processes and produces reasonable results. So far, the performed analysis demonstrates the sensitivity of a very limited model. It is very surprising that the authors have used this limited model, since a more sophisticated version of PFLOTRAN for permafrost seems to be available (Karra et al.,

2014). Therefore, it is very questionable if the results can be used for general down- or upscaling procedures as claimed in the manuscript. The suggested scaling procedures will be certainty problematic under climate warming and permafrost degradation scenarios since thawing and freezing processes are not included.

V. In addition to the well cited US literature, there is a wide range of studies available concerning hydrological processes and surface heterogeneity in polygonal tundra landscapes also for important Russian field sites. I suggest to have a closer look at these studies as well (e.g. Cresto Aleina et al., 2013).

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