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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 #1

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I believe that this work is flawed in a manner that cannot be easily corrected. My major concern is as follows. The authors have found that for a patterned ground site at Barrow, Alaska, there exist spatial scaling relationships for statistical moments of soil moisture. However, their results have been determined entirely within the construct of their particular modeling framework (FLOTRAN). The authors have not demonstrated that these model-based relations translate to actual (observed) soil moisture scaling. No comparisons between modeled results and results obtained though measurements are presented. All results are from simulation . I fully admit that measuring soil mois-

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ture at high resolutions (a 25 cm grid) and over reasonably large regions is difficult at best. Nevertheless, without some verification that the model generated soil moisture distribution correlates with observations, these results are intriguing at best.

There are other reasons to suspect that the findings presented in this work may not carry over to the real world. Specifically, a number of possibly critical simulation assumptions are made without defense as to their validity. These assumptions include 1) Isothermal flow. Is this a good assumption - or at least good enough so that "continuous summer" simulations are valid? 2) The thermo-physical properties of the soils are assumed to be uniform in all spatial dimensions. Clearly this is not the case, especially so in a site with patterned vegetation? The soils under vegetation will be different than the soils in bare areas 3) The vegetation cover is considered spatially uniform. This is a polygonal patterned site where the highly insulativive properties of the vegetation may lead to significant spatial heterogeneity in subsurface soil temperatures (especially for the 25 cm simulations) - possibly violating the isothermal flow assumption. None of these assumptions are defended in any manner.

In summary, I have two major concerns 1) No convincing argument is made that the simulation results found in this study will translate to the actual landscape. 2) A number of simulation assumptions are presented without any discussion as to there adequacy. I believe that some the latter concerns can be addressed through discussion and simple simulations. However, I do not believe that the former concern can be addressed without soil moisture validation results. It would be curious to know wether the same results found in this study could be had without simulation - specifically, by taking a reasonable mid-summer water table depth and the spatial distribution of the Topmodel Topographic Wetness Index (derived from the 25cm - 8 m DEMs) and then simply calculating the spatial distribution of surface soil moistures using Topmodel equations.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 12833, 2014.