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Interactive Comment

Interactive comment on "A reduced order modeling approach to represent subgrid-scale hydrological dynamics for regional- and climate-scale land-surface simulations: application in a polygonal tundra landscape" *by* G. S. H. Pau et al.

Anonymous Referee #2

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This study presents the application of a reduced order modeling (ROM) approach to downscale PFLOTRAN simulation from relative coarse resolutions (e.g., 8mx8m) to obtain prediction at finer resolution (e.g., 0.25mx0.25m). I appreciate the value of the ROM approach as it helps to drastically reduce the computational load and at the same time achieve as much as possible high-resolution predictions. As such, I do see great potential of such an approach to be applied to earth system models in general and CESM in particular.

However, after reading through the manuscript I am having a hard time to accept the authors' assertion that "This method has the potential to efficiently increase the resolution of land models for coupled climate simulations, allowing LSMs to be used at spatial scales consistent with mechanistic physical process representation." Also, I am not convinced that authors have adequately demonstrated the feasibility to transfer and generalize this type of ROM approach to diverse climate and landscape conditions at the regional or global scales. These being said, my specific comments are listed as below.

1. The ROM approach has not been actually demonstrated with an actual Land Surface Model. This study is primarily based upon the numerical simulations facilitated by PFLOTRAN model, which is traditionally a subsurface flow and biochemistry model. The fact that the authors added a overland routing component does not make PFLOTRAN automatically a Land Surface Model. Community Land Model (CLM4.5) is used to provide boundary conditions to PFLOTRAN only. Thus a better linkage of PFLOTRAN to LSMs is needed.

2. The model simulation resolutions covered in this study, from 0.25m to 8m, are far smaller than the typical resolutions adapted in LSMs, i.e., 1km and coarser. Actually even the applications of LSMs at 1km resolution at the regional scale are very rare. Given that different types of subgrid heterogeneity dominate at different ranges of spatial resolutions, it is not straightforward to transfer our understanding gained from the simulations at 0.25m-8m resolutions to the typical LSM resolutions.

3. Judging from the methodology part itself, it is hard to tell whether the ROM approach utilized here is a new type of ROM method proposed by the authors, or just application of established ROM approach to a new sets of numerical simulations. If the latter is the case, I would encourage the authors to invest a bit extra effort to eliminate the seemly overlapping between the current manuscript and another one submitted to HESS (Bisht and Riley, 2014), which is based on the same set of numerical simulations used in the current study and, moreover, largely discuss downscaling coarse-resolution simula-

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tions to the fine-resolution using topographic information. This is not a major concern though and some extra clarification will do.

Bisht, G. and Riley, W. J.: Topographic controls on soil moisture scaling properties in polygonal ground, Hydrol. Earth Syst. Sci. Discuss., submitted, 2014.

4. P2133, Line 8-14. Why do you limit p to be time and the climate forcings only? Please specify your rationale. P2137, Line 15-25. The physical mechanism(s) behind the so called "increasing hydrological complexity" are not clearly revealed. The distributions or standard deviations of the DEMs are certainly not the best angle to interpret. There are many places where the land surface topography can be characterized by structured/organized heterogeneity, e.g, polygonal landscape in this case. This kind of structured heterogeneity can by no means be captured by the distributions since the same PDF could be extracted from a completely randomly heterogeneous field and another one with some structured heterogeneity. Is it possible that, from A, B, C to D, the dominance of topography (polygonal structure) is less and less, thus more and more other factors such as soil, vegetation and forcings start to play a bigger role (as captured by increasing value of M)?

5. How would the performance of ROM approach vary under different landscape (topography, soil, vegetation etc.) and boundary conditions (prec., snowmelt, ET etc.)? A good understanding of this issue will enhance the confidence to generalize the ROM approach to other climate regions.

6. The past tense makes the article less readable, as also pointed out by Reviewer #1. For example, the sentence at P2134, Line26-27 sounds almost weird.

7. P2137, Line 13. M values increase with decreasing error? Please rephrase.

Interactive comment on Geosci. Model Dev. Discuss., 7, 2125, 2014.