Reply to referee #2.

We thank the referee for the positive comments. Below are our responses (in normal font) to reviewer's concerns and questions (in italics).

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.

A: We agree that we have not applied the technique described to areas of size and diversity typical of climate simulations, but re-emphasize that that was not the goal of this paper. As discussed in the manuscript (pg. 2142 line 1), we view the development and applicability of ROMs on a relatively small spatial (1,000 m^2) extent as only a first step towards representation of processes within LSMs at climate-scales. To further clarify this restriction of the current work, we have removed the "regional- and climate-scale" from the title and have presented an expanded discussion on how our current results could be extended to larger-scale simulations in Section 3.4: Application to larger-scale hydrological simulation.

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.

A: We agree we did not use a land surface model typically used in climate simulations, but re-emphasize that that was not the goal of this paper. For the Arctic tundra sites that are studied in this manuscript, PFLOTRAN with overland flow is a suitable model due to relatively small topographic features at our sites and the desire to model 3D flow. In addition, there is a greater need for developing ROMs for models that cannot be embarrassingly parallelized, such as the 3D flow model we studied in this manuscript.

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.

A: Generalization of the current approach to 1 km extents and length-scales relevant to BGC heterogeneity defines a research goal that we are currently actively addressing, but is beyond the scope of the current manuscript. To address this reviewer concern, we have added additional text discussing how we intend to approach the transferability and general applicability of the proposed ROM approach within LSMs (Section 3.4). In particular, we are currently developing a hierarchical approach that involves using POD-MM methods to develop ROMs at multiple scales. After the construction of ROMs, the POD-MM procedure is then recursively applied to reconstruct solution at progressively finer scale, starting from the coarsest scale solution. For other details, please see our responses to Referee #1.

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.

A: The ROM method we describe is similar to a technique first reported in Robinson et al. (2012). In this manuscript, we demonstrate, for the first time, how the same technique can be applied to a hydrological model. We address issues related to using the POD-MM method, such as determining the optimal number of POD bases to use. We also describe multiple modifications to the basic approach that improve the efficiency and accuracy of the resulting approximation. To address this referee concern, we have reorganized the manuscript such that the methods we have developed are now described under one section (Sect. 2) to more clearly highlight our contributions.

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 simulations 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.

A: We understand the reviewer's concern. We have taken care to differentiate our work from the HESS paper mentioned above. In summary, the two papers took completely different approaches to upscaling. The manuscript under review here used a numerical approximation approach as opposed to the HESS paper that focused on a statistics-based approach.

4. P2133, Line 8-14. Why do you limit p to be time and the climate forcings only? Please specify your rationale.

A: Time and magnitude of the two climate forcings (evapotranspiration and net precipitation) are the controlling factors that vary in the set of simulations that were performed.

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)?

A: In this model, the primary source of heterogeneity in the model is topography. While soil, vegetation, and forcings could be factors influencing the size of *M*, these factors are not studied in the set of simulations that we have performed. Thus, the "hydrological complexity", i.e. variability in the soil moisture solutions over times and sites, may only be linked to the topography and the roles of the other factors cannot be deduced from these simulations. Contribution of other factors are currently being addressed using a different model but is beyond the scope of the current manuscript.

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.

A: The site-independent ROM described in Sect 3.3 provides an indication of the performance of ROM when it is generalized to handle different topographic region. Compared to site-dependent ROM, there is a reduction in accuracy for site-independent ROM. However, the results can be improved if more domains with different topographic characteristics are taken into consideration during the construction stage of the ROM. We added 1 paragraph to Sect. 3.3 and 3 paragraphs to Sect. 3.4 to address this comment.

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.

A: The sentence should really be "However, we present results …". Unless the editor indicates otherwise, we prefer to use past tense for work that happened in the past, and present tense for discussion and explanation of ideas resulting from work that happened in the past and suggestions for future work. To address this reviewer concern, we have reviewed the manuscript carefully to ensure that these guidelines are followed.

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

A: The line should read "The M values at which we evaluate $\bar{e}^{\mathrm{POD}}$ correspond to decreasing $\varepsilon^{\lambda} = 10^{-1}, 10^{-2}, \ldots, 10^{-8}$ in equation (3).