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Interactive comment on "A subbasin-based framework to represent land surface processes in an Earth System Model" *by* H.-Y. Li et al.

H.-Y. Li et al.

hongyi.li@pnl.gov

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Although the overall subject area of this paper (better representation of spatial heterogenity and hydrology in large-scale models) is of interest, I found little interesting information in the manuscript and recommend that it be rejected. Overall it is well-written, although some of the figures could be improved through the use of alternative colour schemes. My main criticism though is that the paper does not describe any substantial advances in the field. The paper describes a comparison between two versions of the CLM land surface model, the first using a traditional 'rectangular' grid and gridboxes, the second being a newly-developed approach in which the basic units are catchments (rather than rect- angular grid boxes). These versions are called CLM and SCLM respectively. The perceived benefits of the SCLM approach include that it

better represents the natural catchment-based structure of a landscape and is more similar to the catchmentbased approach used in the hydrological modelling community. The study considers the Columbia River in the U.S.A. and compares results for CLM using 0.125_ gridboxes with those from SCLM using catchments of approx 0.125_ size. GIS methods are used to prepare appropriate input data for SCLM by calculating catchment-average values. The main result is a modest change between CLM and SCLM in the simulated streamflow. The authors have clearly done a substantial amount of work in setting up the simulations and in creating the input data, and the system they describe has the potential to yeld some interesting results. However, on the basis of this paper I would say that they do not yet have those results. By choosing to use 0.125_ catchments for SCLM (on the basis that this is the fairest first comparison) the authors have almost inevitably found little difference between the models (as noted by Reviewer 1). It is quite possible that larger and more beneficial changes can be found if SCLM uses smaller units, but this was not tested here. Similarly, calibration for SCLM (which currently uses parameter values obtained for CLM) will most likely improve results. The authors also suggest (Summary, p2714 line 22) that SCLM is less sensitive to changes in resolution, which is another promising result but not from the current study. I see lots of promise, but not enough in this paper. Response: We thank the reviewer for these major concerns. Even though we agree with the reviewer that the paper is not scientific in nature, we would like to point out that given the journal's focus on model development, we submitted this paper more to illustrate the feasibility and technical details of how to apply a land surface model within an Earth system modeling framework in a way that is typical in hydrologic modeling. As discussed in the paper, up to now, there are only limited attempts (i.e., Koster et al., 2000; Goteti et al., 2008) toward this direction, each subject to its own limitations.

This study constitutes technical advances over these previous studies in two ways: 1) deriving input forcings and land parameters from high resolution datasets; and 2) coupling a new physically based river routing model in the subbasin-based framework. Even though these are again, typical in the field of hydrology, we argue that they are still new and innovate in the field of land surface modeling, or broadly, climate and earth system modeling, especially given the increasing complexity in software engineering in these models. As discussed in the summary section of the paper, CLM4 is the land component of the Community Climate System model and the Community Earth System model (i.e., CCSM4 and CESM1), which contain other components of the earth system (i.e., atmosphere and ocean). In order to ensure proper coupling between the component models, it is critical to make sure that discretization of each components is carefully designed so that mapping between the components could be achieved. In fact, the capability of using unstructured grids (such as the subbasin boundaries) in component models of the CCSM/CESM has only been recently achieved through the design of the coupler CPL7 (Craig et al., 2012), which makes the subbasin-based execution of CLM4 feasibility. And to our knowledge, a subbasin-based application of CLM4 has never been achieved by anybody else.

Nevertheless, as pointed out by the reviewer, we failed to clearly explain the indexing system, which is critical for understand the coupling between SCLM and MOSART. We will add detailed discussion to the revised manuscript if we are offered a chance.

In addition, as pointed out by reviewer, soil depth is a notoriously difficult input parameter for hydrology modeling. It is, however, greatly simplified in CLM4 by assuming that soil depth has a globally uniform value of 3.8m (Oleson et al, 2010). Even though it is a bold assumption, it is typical in the field of land surface modeling due to lack of soil depth data globally.

However, we have decided to redirect our study to focus on larger and more beneficial advantages of the SCLM approach as suggested by the reviewers. We strongly agree with the reviewer that there are a lot promises in this area and therefore would like to continue our study. We have made a revision plan in which we have strong confidence, for more details please refer to our letter to the editor.

Specific comments There is fairly extensive discussion of how the input data were pre-

pared (e.g. using GIS) but these do not appear to be novel (essentially using weighting by area). Response: This will be condensed into the appendix part as we see them as useful reference to some readers who are not familiar with these procedures.

The “important hydrologic parameters” fmax and Cs are not properly introduced - the reader should be given some idea of what they are and how they work. Response: We will provide more introductions of these hydrologic parameters.

Several of the figures (at least 2, 3, 4, 6) contain fairly little information - essentially they show that differences between approaches are small, which arguably might be better just stated in the text. Response: These figures will likely be completely removed.

The colour scheme of Fig,4 (essentially blue everywhere) is not helpful. Response: We will pay great attention to the sharpness of the figures.

To me one of the features of Fig.5 is that the SCLM-calculated streamflow is often worse (compared to observations) than the CLM streamflow (e.g. low bias in June is often worse), which should at least be acknowledged in the text. I accept that this is a preliminary examination and SCLM might in time produce better results. Response: This will be noted in the discussion.

Various conclusions (e.g. that using high-resolution DEMs to calculate topographic indices does not guarantee improvements in simulated hydrology - in the Summary) are not all surprising to those in the field and do not serve to increase confidence in the paper. Response: This part will be largely rewritten.

The paper spends quite a while looking at different precipitation regimes and looking into the details of the hydrological simulations and forcing data, but my impression is that they are looking at small details (which might have to be done, but not reported at such length when the overall changes and conclusions are so slight) in the absence of major results. Response: We will shorten the interpretation/discussion of these details.

References:

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Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/6/C1206/2013/gmdd-6-C1206-2013-supplement.pdf

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Interactive comment on Geosci. Model Dev. Discuss., 6, 2699, 2013.