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Hydrology and Earth System Sciences Discussions

Interactive comment on "Parameterizing sub-surface drainage with geology to improve modeling streamflow responses to climate in data limited environments" *by* C. L. Tague et al.

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This manuscript introduces a strategy to regionalize subsurface drainage parameters based on geologic classification (or similarity). The case study area, the Mckenzie River basin, is divided into two distinct geologic regions, the Western Cascades (WC) and High Cascades (HC) regions. The key assumption is that the geologic classification is a first-order indicator of subsurface drainage parameters within a distributed hydrologic model, i.e., those subcatchments which are geologically similar can be represented by the same set of subsurface drainage parameters. This hypothesis has

been validated via the application and calibration of a physically based, distributed model. Overall it is interesting but still could be improved. I would recommend a moderate revision before acceptance for publication based on the following more specific comments.

1. The hydrologic responses, and thus parameters within a hydrologic model, are usually controlled by both climate conditions and landscape properties. It is stated, based on previous studies, that "within the McKenzie, geologically mediated spatial differences in subsurface drainage characteristics can be a 1st order control on spatial patterns of streamflow response to warming". But the role of climate conditions, as comparing to geological differences, has not been well articulated. There could be some adequate analysis/discussion on the impacts of spatial patterns of meteorological forcings. Are the authors implying that climate conditions are of less importance here? In any case, it should be better clarified in the introduction part at least. Moreover, did the authors check the spatial variation of precipitation and temperature based on measurements from multiple meteorological stations (even though most of them may not have long records)? How does the spatial variation of precipitation derived from ground observations compare with that from PRISM grids?

2. Many readers may not be familiar with the RHESSys model. The model structure itself deserves more description here, particularly the watershed hydrology part, and then the readers will have a better understanding of the corresponding parameters. More importantly, this study is based on the premise that the model structure is sufficiently adequate to capture the major hydrologic processes within this mountainous, snow-dominated region. How about some diagnosis on the model structure itself? For example, how much will the model performance improve if turn on the deep groundwater component for WC regions? How much will the model performance (and other related parameters) change if turn off the deep groundwater component?

3. I am wondering how the total runoff is partitioned into different components, i.e., surface and subsurface runoff. For instance, in Figure 4, instead of showing total runoff

(streamflow) simulation and observation for different periods, more insight would be gained by showing and examining inter- and intra-annual variability of the relative contribution of surface and subsurface runoff. This might provide better justice for the latter usage of spring fraction of annual flow since in this season subsurface runoff is likely dominating.

4. One thing might be interesting to look at the effects of spatial resolution on parameter calibration. How the subcatchments are delineated is missing. Will the values of behavioral parameters change after calibration at different levels of delineation? The same question goes with the predictive power of the behavioral parameter sets.

5. Above said, I also encourage the authors to pay attention to some minor issues: a. The two terms “watershed” and “basin” are used in a mixed way (e.g., lines 21 and 23, page 8669). To many others they might have different definitions. It would be better to use one of them consistent throughout the content or state a priori that they are considered exactly the same in this manuscript. b. Line 26, page 8667-line 2, page 8668. It is a known fact that “the potential error in applying calibrated parameters across an entire watershed”. This should not be a major contribution of this paper. Please rephrase. c. Line 7-8, page 8674. What’s the rationale of choosing these two performance metrics? Please add a couple of sentences here. d. Line 5-9, page 8676. The sentence does not read well. Please rewrite.

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