Hydrol. Earth Syst. Sci. Discuss., 9, C4888–C4892, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C4888/2012/

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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Received and published: 18 October 2012

We would like to thank Dr. Li for the thoughtful review and helpful comments regarding this paper. Our responses to the reviewers comments are below.

1. We strongly agree with the reviewer that estimation of climate forcing, and in particular spatial interpolation of point meteorologic inputs, is a key source of model error and uncertainty. In this paper, we intentionally used a consistent approach for estimating climate inputs to the model for all watersheds. We acknowledge however that spatial patterns of error in climate input influence model estimates and their respective

spatial pattern. We have added the following to our discussion of model performance to acknowledge this:

"There are, however, notable differences in inter-annual mean and variation between observed and modeled estimates. One potential source of these errors would be errors in estimation of meteorologic inputs. Interpolation of both temperature and precipitation in mountain environments is a well-documented source of error in hydrologic models (Liston and Elder, 2006). Here we use a relatively simple approach where point meteorologic measurements of temperature are scaled using a constant environmental lapse rate of temperature with elevation, and precipitation is scaled based on long-term mean patterns derived from PRISM (Daly, 1994). Recent studies have shown that air temperature lapse rates with elevation are considerably more complex in this region, reflecting temperature inversions and cold air pooling (Lundquist and Cayan, 2007, Daly et al., 2007). Similarly, there are likely to be substantial errors in interpolating precipitation data for specific storm events. Our use of daily streamflow over several decades for model calibration and evaluation emphasizes long-term seasonal patterns of high and low flows and recession behavior – which are more likely to be sensitive to average climate and geology and are the focus of this paper. We therefore emphasize drainage parameter calibration and transferability, given expected uncertainties in meteorologic forcing. What is particularly encouraging is that even with these limitations the SF watershed shows no degradation in performance relative to calibrated watersheds (based on predictions of spring fraction of flow). Future work will focus on disentangling the relative roles played by errors in meteorologic forcing and drainage properties."

We argue however that evaluation of methods for interpolating meteorologic inputs is beyond the scope of this paper (e.g by comparing interpolated estimates of precipitation or temperature with measurement). This could be (and has been) the subject of a stand alone paper that is under review in WRR where we examined the sensitivity of model hydrologic estimates to different temperature interpolation approaches (a summary of findings was also published as an AGU abstract).

2. As requested we have added some additional detail on process representation in RHESSys. We also note that we cite a number of earlier papers that document comparison of RHESSys estimates with observations of streamflow, snow, and vegetation carbon and moisture fluxes in mountain regions – and added the following for clarification:

"RHESSys has been applied and evaluated against observed streamflow, snow and carbon and moisture flux data for a number of mountain catchments in the western U.S., (Baron et al., 2000; Tague and Grant, 2009) and mountainous catchments in Europe (Zierl et al., 2006)"

3. We have added some additional text on how RHESSys partitions runoff and the relationship with calibration parameters. We note that surface runoff rarely occurs in these watersheds, and all flow is dominated by shallow subsurface or deeper groundwater fluxes. We also note that spring fraction includes both deep ground water and shallow subsurface responses. For WC sites, we select parameters such that ALL flow is through shallow subsurface flow systems and there are still substantial spring flows. We focus on spring fraction in this paper because it is a commonly used streamflow metric for assessing climate warming impacts in the Western US. We acknowledge that some confusion may have arisen due to our overly brief explanation of parameters and we have expanded our description.

"The gw1 and gw2 parameters are used to characterize the deeper ground water systems that are well below biological active soil and rooting zone. The other 4 parameters (po, pa, m, K) reflect soil characteristics and shallow subsurface flowpaths. We hypothesize that the younger, deeper groundwater dominated HC region will lead to higher values of gw1. We also note, however, that soil water-holding capacity (parameters po and pa) and shallow subsurface drainage (m and K) are also likely to depend on the time taken for soil development. Western Cascade soils are derived from bedrock that has weathered in place for up to 30 million years over which time a wide range of clay species have developed forming impervious layers and aquacludes. Infiltration

rates are high with abundant residual stones and clasts (Dyrness, 1969), and soils are shallow due to mass wasting and creep. HC soils in contrast are much younger (less than 7 million years), and typically lack abundant clays and corresponding impermeable layers. They also occupy much lower gradient portions of the landscape, meaning that hydraulic gradients are gentler."

4. We agree that examining the effect of spatial resolution on parameter selection would be an interesting study but argue that it is beyond the scope of this paper.

5. Minor issues listed:

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 deïňĄnitions. 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.

We replace all uses of "basin" with "watershed" for consistency.

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.

We have removed this sentence

c. Line 7-8, page 8674. What's the rationale of choosing these two performance metrics? Please add a couple of sentences here. We added the following:

"The Nash Sutcliffe Efficiency is a commonly used metric for evaluating streamflow predictions from hydrologic models. Because streamflow in this region has a high dynamic range (high winter peaks and low summer flows), we add the NSE of log-transformed flows to test whether model can capture recession and summer flow behavior as well as storm flows."

d. Line 5-9, page 8676. The sentence does not read well. Please rewrite. We re-wrote as follows:

“Relative to the WC watersheds, the HC watershed CLR shows improved performance for higher values of gw1, lower values of gw2, higher values of m, and lower values of K. This set of parameters for a HC watershed reflect a slower draining system with greater proportions of infiltrated water connecting to a deeper groundwater reservoir.”

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 8665, 2012.