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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.

M. Gannett (Referee)

mgannett@usgs.gov

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Efforts to use water balance models to simulate the hydrologic response to climate projections are hampered by the general inability of such models to simulate groundwater. This is particularly problematic in volcanic basins where streamflow has a large component of groundwater (for example see the discussion pertaining to the Deschutes Basin on p 78 of Brekke and others, 2010). This paper begins to address this issue by proposing a scheme to use a "geologically-based parameter transfer scheme" (presumably based in part on surface geologic mapping) so the subsurface hydrogeologic

characteristics can be represented in models. The ideal solution to this problem, of course, is to explicitly simulate subsurface flow in coupled groundwater/surface-water models, but suitable groundwater models have been developed for only a handful of areas. This paper is an important contribution to the evolving discussion of how to improve our ability to simulate the groundwater component of hydrographs over broad regions using widely available information.

The modeling work and the presentation are technically sound and I found no fundamental scientific problems. I offer some general comments that I hope will improve the discussion, followed by some comments regarding specific parts of the manuscript.

General comments:

It would be good to include more discussion on generalizing the findings of this work. The study area for this work spans two highly contrasting hydrogeologic settings. How can this work be applied where hydrologic contrasts are more subtle or where no "end member" drainages are present?

Not all readers will be steeped in Cascade Range geology, so some additional discussion on the hydrologic differences between the Western and High Cascade provinces and the specific geologic map criteria used to discern them might be helpful. What insights gained from this work can be applied in areas where available surface mapping units do not correspond so profoundly with hydrologic behavior of streams?

The authors are careful not to apply to HC watersheds their finding that parameter sensitivity in WC watersheds was similar across scales. I think some explicit discussion of the possible relation between groundwater parameters and scale in HC watersheds may be appropriate. The work of Manga (1996, 1999) relating the response of springs to aquifer scale suggests that your gw2 parameter might be correlated with drainage basin size in groundwater-dominated basins.

Specific comments:

In the first sentence of the Methods section the RHESSys model is described as spatially distributed. It would be helpful to include a sentence describing the spatial structure of the model so the reader doesn't have to search out the provided reference.

Regarding the RHESSys model parameter descriptions on page 8673, it would be good to provide dimensional units for the parameters. This is particularly important for "m" and others critical to later parts of the discussion. Does a large value of "m" mean that K diminishes more rapidly with depth?

Line 17, p 8674 – What criteria were used to select the four calibrated parameter sets from the generally acceptable dataset?

Last sentence of p 8675 uses the term "sensitivity" where the figure caption uses the term "preference".

Results section top of page 8676 – It would be good to add discussion relating the results back to the hydrogeology of the WC and HC landscapes. For example, the finding that the CLEAR (HC) watershed showed improved performance with lower values of K and higher values of m (which I assume means that K decreases more rapidly with depth) relative to the WC watersheds seems counterintuitive. I would expect HC watersheds to have thinner and less well-developed (i.e. more permeable) soils than WC watersheds.

Line 15, p 8677 – Begins the sentence: "Thus, for the HC watershed a deeper groundwater store must be included based either on the initial or more stringent criteria for parameter selection." But according to table 2 it appears that no HC watersheds met the more stringent criteria.

Second paragraph of page 8878 – The 20 percent bias in the simulated streamflow is attributed to error in precipitation inputs. Could the bias possibly be related to the commonly poor correspondence between groundwater and surface water catchment areas and the tendency for subsurface flow between drainages in the High Cascades?

This would be worth mentioning I think.

Line 4, p 8682 – It would be worth pointing out that the results described mirror the findings of Mayer and Naman (2011) based on analysis of historic data.

Finally, I found figure 3 to be not very intuitive and difficult to understand. An expanded discussion on how this figure was generated (and addition of units) would be helpful.

References Cited:

Brekke, L., Kuepper, B., and Vaddey, S., 2010, Climate and Hydrology Datasets for Use in the RMJOC Agencies' Longer-Term Planning Studies: Part I - Future Climate and Hydrology Datasets: http://www.usbr.gov/pn/programs/climatechange/climehydroRMJOC/index.html, accessed September 2011, 183 p.

Manga, M., 1996. Hydrology of spring-dominated streams in the Oregon Cascades. Water Resources Research, 32(8): 2435-2439.

Manga, M., 1999. On the timescales characterizing groundwater discharge at springs. Journal of Hydrology, 219(1-2): 56-69.

Mayer, T.D., and Naman, S.W., 2011, Streamflow response to climate as influenced by geology and elevation: Journal of the American Water Resources Association, (JAWRA) 1-15. DOI: 10.1111/j.1752-1688.2011.00537.x

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