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Interactive Comment

Interactive comment on "A geohydrologic framework for characterizing summer streamflow sensitivity to climate warming in the Pacific Northwest, USA" *by* M. Safeeq et al.

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We thank this reviewer (Anonymous Referee #3) for providing detailed comments (RC) on our discussion paper. Below are responses (AC) to the comments: (RC1) P3320, Section 3, A complete formulation of equation (1) takes the following form: dS/dt = IR + IM + GWIN – ET – Q – GWOUT where GW is groundwater. The authors neglected to include groundwater terms in their water balance approach. Given the susceptibility of mountain catchments to inter-catchment groundwater exchange I find it dubious to omit a GW term, especially considering work from Jefferson et al. (2006) demonstrating the inability of topographically defined watersheds to describe aquifer boundaries

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Interactive Discussion

Discussion Paper

within the Oregon Cascades. It would be helpful for the authors to discuss the potential influences of inter-catchment groundwater exchange on estimates of streamflow sensitivity and how this complicates mapping streamflow sensitivity to the natural landscape. (AC1)This is an excellent point raised by this reviewer as well as reviewer #1. We will include the groundwater term in equation 1 and discuss the potential influence of inter-catchment groundwater exchange on streamflow sensitivities in the revised version of the manuscript. As we mentioned earlier in response to reviewer #1, these inter-basin transfers are extremely difficult to quantify or even estimate at the landscape scale. Our discussion will focus on how the potential inter-basin transfer via groundwater represents a source of error, and that this error is likely to be greatest in basins with a substantial groundwater component (i.e., areas with deep volcanic aquifers such as the High Cascades). We don't expect, however that neglecting this potential source of error will change the overall pattern of sensitivities. Our approach is not intended to predict total streamflows but show how sensitivity to climate change is spatially distributed. But we agree with the reviewers that this key issue deserves discussion. (RC2) P3326, Section 4.1.2, Reporting adjusted R squared metrics ranging from 0.43 to 0.58 as reasonably accurate may be misleading, I would prefer the authors simply offer the values and allow the reader to judge their accuracy. Also the statement that "Irrespective of geographic domain (OR, WA or both combined), it is apparent that the regression models provide estimates of k with reasonable accuracy" seems speculative. If possible, the authors conjecture should be supported by citations of existing work where metrics/estimates of drainage efficiency (such as k) may have been evaluated using other techniques. (AC2)A similar concern was also raised by Referee #1. We will make the suggested changes in the final revision.

(RC3) P3330 Section 6.1 Comparing St to "T is difficult, I encourage the authors to think of other possible metrics for comparing empirical results to analytically derived ones such as (ST). (AC3)We did not compare the streamflow sensitivities St or SQ0 to T (temperature) or P (precipitation). Rather, we used the streamflow elasticity (see eqn. 10 & 11) to changes in precipitation and temperature as a way to validate our approach. Streamflow elasticity-based metrics have been used to describe watershed sensitivities to climate change (e.g., Fu et al., 2007; Safeeq and Fares, 2012; Vano et al., 2012) and overall hydrologic regime (Patil and Stieglitz, 2012; Schaake, 1990, Sankarasubramanian et al. 2001, Sawicz et al., 2011). We have explored other metrics (e.g. low flow, slope of low flow to annual precipitation, monotonic trend in summer flow etc.) for validation purposes and found very similar results. To avoid introducing a new set of metrics, we decided to use the elasticity approach, which is well established in the literature. (RC4) These groundwater-dominated landscapes in effect "remember" changes in climate as reflected in either the magnitude or timing of recharge in the winter or spring, resulting in higher sensitivity of late-season streamflow. The authors refer to groundwater dominated catchments and their "memory" to climate; this has been noted by Godsey et al. (2013) where summer low flows within certain Sierra Nevada, CA catchments exhibited significant correlation to the previous year's snowpack (i.e. summer low flows do not only depend on the current Q0). Because of how Q0 is defined (equation 2), it neglects to incorporate any "memory" effect from previous recharge events. Given the potential for catchments within the authors' study area to exhibit these "memory" effects it would be beneficial for the authors to acknowledge the limitation of Q0's current definition and to discuss how their framework could incorporate additional metrics to evaluate potential "memory" effects. (AC4)We agree with the reviewer that in its current form our sensitivity framework does not account for the "memory" effect. To do so would require a re-formulation of Q0 to account for this inter-annual interaction. We would expect this effect to be most pronounced in areas with either 1) late melting snowpacks, hence carryover of soil moisture from year to year; and 2) areas with slow-draining groundwater, i.e., deep volcanic aquifers. We will ensure that the model limitations are highlighted in the revised manuscript. (RC5) P3337 Section 8, the authors refer to a "geoclimatic framework" whereas the title and elsewhere in the article use the term "geohydrologic framework", choose one term and be consistent throughout. Typos P3326 L4, ": : : variables are used predict k,: : :" please insert "to" between "used" and "predict". P333a L7, Please remove the word “in”. (AC5)We will incorporate the suggested changes in the revised manuscript.

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