

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

Anonymous Referee #1

We thank this reviewer for providing detailed comments on our discussion paper. Below are responses to the main issues:

(1) The authors state in the introduction section (P3318, L20): “The uniqueness and strength of this approach is that it is independent of climate change scenarios. Sensitivity is mapped as an intrinsic property of the landscape, rather than a response to climate change”. However, given how their conceptual model has been formulated (Section 3), the sensitivity metrics are dependent on Q_0 (equation 6), which in turn is dependent on rainfall, snowmelt, and ET (equation 2). Based on equation 2, it is fair to assume that Q_0 would be responsive to climate change and would make sensitivity responsive too. How can one then claim that sensitivity is an intrinsic property of the landscape and not a response to climate change? A better explanation is needed from the authors as to why they consider streamflow sensitivity to be an intrinsic landscape property.

This is a very interesting point raised by the reviewer which requires some clarification. By “intrinsic property” we meant “intrinsic hydrogeologic property” and not just physiographic property. We agree that Q_0 (both timing and magnitude) would be responsive to climate change; this is, in fact the basis for our sensitivity framework. However for baseline conditions, we have calculated the average Q_0 using historical (1916-2006) rainfall and snowmelt (see Eqn 8 & 9), assuming that this 91 period is long enough to satisfy the stationarity assumption. We assumed that this 91 year average Q_0 along with k are “intrinsic hydrogeologic properties” and unique to each landscape.

(2) For the purpose of mapping the streamflow sensitivity metrics (Figure 8), Q_0 is estimated from either the rainfall (IR) or snowmelt (IM) amount as described in Section 4.2. An implicit assumption in doing so seems to be that the watersheds are responding to climatic inputs that occur only within their own boundary. However, results from recent studies in the PNW (Wigington et al., 2013; Patil et al., 2013) suggest that streamflows in some watersheds, especially in and near the High Cascades, could be significantly influenced by groundwater gains/losses from outside of the watershed boundary. This not only complicates the characterization of this connection between climate inputs and streamflow outputs, but also increases the uncertainty likelihood of streamflow sensitivity predictions in those regions. It would be helpful if the authors can provide some discussion on the limitations caused by substituting Q_0 with IR or IM in their conceptual model.

We agree with the reviewer that in this region groundwater gain and loss from outside HUC units could potentially influence our sensitivity analysis. A similar concern was also raised by Referee #3, who suggested modifying equation 1 and including a term for groundwater gain and loss (see modified section 3 in the main text). In our view, physically accounting for groundwater gain and loss in this conceptual sensitivity framework with little or no data to draw on will undermine

the simplicity of the paper and not significantly affect results (see comments to Reviewer 3). In addition to modifying the equation 1 as suggested by Referee #3, we have added to the discussion the limitations of our approach (see last paragraph on page 6).

(3) A better explanation of Section 4.1.2 is needed, especially for the second paragraph. From my understanding, the authors first developed the regression model based on the data of 227 catchments (Figure 4) and then extrapolated it to the HUC scale watershed boundaries. However, the authors have not explicitly stated this transition from model development at catchment scale to the extrapolation at HUC scale in their paragraph.

We have made the suggested changes in the final revision. See the revised text in section 4.1.2.

(4) P3326, L6: “Irrespective of geographic domain (OR, WA or both combined), it is apparent that the regression models provide estimates of k with reasonable accuracy (Table 1)”. In my opinion, it is quite a stretch to characterise R^2 values of 0.50 to 0.59 as “reasonable accuracy”. Why not just state the R^2 values and let the reader be the judge of accuracy?

A similar concern was also raised by Referee #3. We have made the suggested changes in the final revision (see the first paragraph on page 9).

(5) Was model validation (Section 5) done at all 227 catchments? If yes, please state it explicitly in that section.

We have used 217 watersheds for validation (Fig 1). We have made the suggested changes in the final revision.

(6) P3330, L23: Please change ‘ragne’ to ‘range’.

We have made the suggested changes in the final revision.

(7) P3337, L20: Please change ‘indentify’ to ‘identify’. On this same line, the authors refer to their framework as ‘geoclimatic’, whereas it is ‘geohydrologic’ in the title and other places in the article. Why not just call it a ‘hydrogeologic’ framework throughout? The mapping of recession coefficient and streamflow sensitivity fits well within the field of hydrogeology.

That is an excellent point and we have made the suggested changes in the final revision.

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

Anonymous Referee #2

We thank this reviewer for providing detailed comments on our discussion paper. Below are responses to the issues:

General comments:

The manuscript presents methodology for characterizing the summer streamflow sensitivity to possible future climate changeability in the Pacific Northwest of the USA. Similar attempts can be found in the literature covering different regions (e.g. Nash and Gleick, 1991, J. Hydrol; Christensen et al., 2004, Climatic Change; Eckhardt and Ulbrich, 2003, J. Hydrol; Milly et al. 2005, Nature etc.). However, the proposed methodology offers new approach, is relatively

robust, practical and could be applied in other areas with the consideration of the possible data availability and scarcity issues. Further, it also combines different aspects of the streamflow recession: the use of sensitivity functions and recession constants. In principle I support the publication of the paper in HESS, but there are some things that need to be clarified, especially related to description of some of the steps while implementing the methodology.

We appreciate the comments and thank the reviewer for pointing out similar efforts in other regions. We acknowledge these parallel efforts (See the second paragraph in the introduction).

According to the specific comments given below, some parts of the paper need improvements in order to enable readers better overview of the implemented methods. In some parts of the paper authors provide additional explanations regarding the used methodology, but as they are provided now, this information are not very helpful (e.g. discussion referring to Fig. 3). I would also suggest some minor restructuring of the paper (e.g. separate, more systematic presentation of data sets used in the paper, now they are presented in section 4.1 and also in discussion sections).

Our goal was to provide readers with some context and justification behind the assumptions used in developing the conceptual model presented in the manuscript. However, we agree that additional explanations regarding the methodologies used can be moved to a separate “model limitation” section under the discussion. Similarly, explanation of some of the data used was included in the discussion section only to demonstrate how the sensitivity values and spatial distributions derived using this framework can be utilized for climate change impact assessments. In the final revised manuscript, the description of all the data used in is moved to section 4.1.

The discussion in section 6.2 and 6.3 is very interesting and points out main results of the data analysis, it is actually a central point of the manuscript. However, my impression is that in some parts it lacks references to figures. It is also quite difficult to follow the discussion if a reader is unfamiliar with specific local geographical conditions. Authors should maybe consider providing some additional information (i.e. basic geological map, basic climate characteristics) in scope of Figure 1. This would also help reader to more easily follow the results shown in Figs. 6, 8 and 9.

We have added additional physiographic and climatic information to figure 1 (panels 1b-d) and now refer to this figure in the text when discussing specific geographies and landscapes.

One very important aspect of the presented methodology is the assessment of the change in the snowmelt recharge. In section 6.1 authors stated that due to the fact that rain dominated watersheds had relatively constant rainfall inputs (IR) and timing of the input (tR), the methodology validation may have restricted to only snow dominated watersheds. This should be also mentioned elsewhere (e.g. abstract). The authors should provide more exact information how the timing of the snowmelt or the delay of the snowmelt after the snowfall was taken into account. The used “VIC” model is briefly mentioned in section 4.2, but there are no information

on the e.g. average delays of the snowmelt after the snowfall. Are the delays also expected to change considerably according to the considered climate change scenarios? This should be more thoroughly discussed, we also suggest presenting such data in combination with Fig. 7.

We have added text in the abstract and elsewhere indicating that model validation was specifically limited to snow melt dominated basins. We are not sure how the delay between snowfall and snowmelt will change under future climate across the region but under a warmer climate we expect faster melt following snowfall. Also, since our sensitivity analysis is driven by snowmelt magnitude and timing as it affects recharge, the timing of snowfall is irrelevant for this analysis.

Specific comments and technical corrections:

1)P. 3321 (Equations 5 & 6): I would suggest changing labels SQ0 and St, one might have a misleading impression that these labels address the catchment storage as given in Equation 1.

The labels for describing the sensitivities have been changed as suggested.

2)Fig. 2: An additional label close to the color scale would help in interpreting the figure.

Labels have been added to the color scale.

3)It seems that Fig. 2 is borrowed from Tague and Grant. (2009). This should be also referred in text not only under Fig. 2 caption.

The Tague and Grant. (2009) citation in the Fig 2 is referring to the conceptual model and not the actual figure. We have changed the figure caption as well as the text for clarification.

4)P. 3322, lines 23-29: Labels t_M and t_R appears to come out of nowhere. What do they stand for?

t_M and t_R represent the timing of I_M and I_R , respectively. Description of t_M and t_R were presented in the text following Equation 8 & 9. We have expanded the relevant text for clarity.

5)Data shown in Fig. 3 should be explained more in detail. You mention delays of t_p vs. t_M and t_R ? How could you distinguish the reported values if you have an ensemble of mean data over the available data series based on 227 water stations distributed over large area? You show peak flows but what are these, long-term average monthly peaks or consecutive real-time peaks? In my view, much more important from the climate changeability point of view are the delays in mean snowmelt and mean streamflow which are apparent, but they are not mentioned at all.

We have tried to characterize the time lags between mean daily recharge timing and mean daily flow peaks by breaking the study domain into three zones -- rain, transitional snow-zone, and seasonal zone. This classification is an accepted way of categorizing the landscape in this region for climate change assessments (i.e., Nolin and Daly, 2006; Jefferson, 2010). We have clarified

the figure caption and text to make it clear what is being plotted in Fig 3. As stated earlier, we did not mention how this time lag between peak recharge (rain or snowmelt) and streamflow will change under future climate for these three zones mainly due to lack of data. We have explored this in detail using simulated data from VIC using 5 GCMs under A1B scenarios. These modeled results indicate that peak melt will shift early in the year as much as by 3-4 weeks at the end of 2080s. Changes in peak rainfall are not conclusive, with some indication of shift towards later in the year by one week. We have added a discussion of how historical recharge and streamflow timing, as shown in Fig 3, may affect the sensitivities described in the paper (see paragraph 2 in the conclusion).

6)P. 3322, lines 26-29: You report an average delay in t_P (day of the peak discharge) of 6 days from t_M (I suppose this is a day of peak snowmelt) for seasonal snow zone watersheds. In rain dominated watersheds, the t_P is lagged behind t_R (I suppose this is a day of maximum rainfall) 9 days. How would you comment these values? This does not seem as an expected hydrological response; one could supposedly expect, that the lags in snow dominated watersheds would be much longer compared to lags in rain dominated watersheds.

We hypothesize that this discrepancy might be due the fact that the snowmelt data used in this analysis were simulated using the VIC model and not based on ground measurement. The VIC model was only calibrated against the streamflow hydrograph, and there is a possibility that the model is not capturing the timing of peak snowmelt accurately. We have explored this further in watersheds where ground measurements were available and results were very similar. The long-delay of 9 days between rainfall and runoff could be attributed to the GW storage depletion during summer. The fall and early winter rain will have to first make for the summer deficit.

7)P. 3324, line 9: Why did you use the exact date 15 August to exclude the impact of snowmelt on k ?

We picked 15 August around which snowmelt in seasonal snow zone approaches to zero and summer baseflow begins (Fig 3). We have clarified this in the revised version of the manuscript.

8)P. 3325, lines 8-18: The meaning of the parameters k_{aqu} and k_{soil} should be more exactly explained as these parameters were further on used in multiple linear regression analysis for deriving k parameter. How was parameter k_{soil} obtained?

This has been clarified in the revised version of the manuscript and we have added the citation for k_{soil} .

9)P. 3326, lines 3-5: The sentence is unclear and needs to be rewritten.

This sentence has been rewritten for clarity

10)P. 3326, lines 10-16, Fig. 4: While discussing the performance of model 2 in predicting the k values, you mention different regions with specific hydrogeological characteristics; however,

these cannot be distinguished from Fig. 4. It would be interesting to see, how good is the model performance related to these regions. You could demonstrate this by using different point colors for each region in Fig. 4. What is Model 1a and Model 1b?

This is very interesting point, We have color coded Fig 4 based on the physiographic region as the reviewer suggests and discuss the results.

11)P. 3338, line 29: The sentence needs grammar revision.

We have rewritten the text.

References

Jefferson, A. J.: Seasonal versus transient snow and the elevation dependence of climate sensitivity in maritime mountainous regions, J. Geophys. Res., 38, L16402, doi:10.1029/2011gl048346, 2011.

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Tague, C. L. and Grant, G. E.: Groundwater dynamics mediate low-flow response to global warming in snow-dominated alpine regions, Water Resour. Res., 45, W07421, doi:10.1029/2008WR007179, 2009.

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

Anonymous Referee #3

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 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 have included 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 now focuses 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 have made the suggested changes.

(RC3) P3330 Section 6.1 Comparing S_t 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 S_t or S_{Q0} 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 have highlighted the model limitations 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 have incorporated the suggested changes in the revised manuscript.

References

- Fu G., Charles S.P., Chiew F.H.S.: A two-parameter climate elasticity of streamflow index to assess climate change effects on annual streamflow. *Water Resources Research* 43(11): W11419, 2007.
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