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Discussions

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 #2) for providing detailed comments (RC) on our discussion paper. Below are responses (AC) to the issues: General comments: (RC)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 el 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. (AC)We appreciate the comments and would like to thanks the reviewer for pointing out to us similar efforts in other regions. We will acknowledge these parallel efforts in the final revision of this paper. (RC)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). (AC)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, we will move the description of all the data used in this manuscript to section 4.1. (RC)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. (AC)As suggested, we will add additional physiographic and climatic information with figure 1 and refer to this figure in the text when discussing specific geographies and landscapes. (RC)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. (AC)In the final revision of this paper, we will add 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: (RC1)P. 3321 (Equations 5 & 6): I would suggest changing labels SQ0 ans St, one might have a misleading impression that these labels address the catchment storage as given in Equation 1. (AC1)We will change the labels for describing the sensitivities during the final revision. (RC2)Fig. 2: An additional label close to the color scale would help in interpreting the figure. (AC2)We will add the labels to the color scale. (RC3)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. (AC3)The Tague and Grant. (2009) citation in the Fig 2 is referring to the conceptual model and not the actual figure. We will change the figure caption as well as in the text for clarification. (RC4)P. 3322, lines 23-29: Labels tM and tR appears to come out of nowhere. What do they stand for? (AC4)tM and tR represent the timing of IM and IR, respectively. Description of tM and tR were presented in the text following Equation 8 & 9. We will expand the relevant text for clarity. (RC5)Data shown in Fig. 3 should be explained more in detail. You mention delays of tp vs.tM and tR? How could you distinguish the reported values if you have an ensample 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 monthy 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. (AC5)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 – that are an accepted way of categorizing the landscape in this region for climate change assessments (i.e., Nolin and Daly, 2006; Jefferson, 2010). We will clarify the figure caption to make it clear what is being plotted in the 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. Comparing the changes in time between snowmelt and streamflow will have to be based on model data which are often not intended to accurately capture the timing of recharge. Given this reviewer's comments, however, we will discuss how historical recharge and streamflow timing, as shown in Fig 3, compares with those simulated under future climate using the VIC model. (RC6)P. 3322, lines 26-29: You report an average delay in tP (day of the peak discharge) of 6 days from tM (I suppose this is a day of peak snowmelt) for seasonal snow zone watersheds. In rain dominated watersheds, the tP is lagged behind tR (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. (AC6)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 will explore this further in watersheds where ground measurements are available and discuss in the final revised manuscript. (RC7)P. 3324, line 9: Why did you use the exact date 15 August to exclude the impact of snowmelt on k? (AC7)We picked 15 August around which snowmelt in seasonal snow zone approaches to zero and summer baseflow begins (Fig 3). We will clarify this in the revised version of the manuscript. (RC8)P. 3325, lines 8-18: The meaning of the parameters kaqu and ksoil should be more exactly explained as these parameters were further on used in multiple linear regression analysis for deriving k parameter. How was parameter ksoil obtained? (AC8) We will clarify this in the revised version of the manuscript and add the citation for ksoil. (RC9)P. 3326, lines 3-5: The sentence is unclear and needs to be rewritten. (AC9)We will rewrite the text. (RC10)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? (AC10)This is very interesting point and we will color code Fig 4 based on the physiographic region as the reviewer suggests and discuss the results. (RC11)P. 3338, line 29: The sentence needs grammar revision. (AC11)We will rewrite the text.

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