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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.

Anonymous Referee #2

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General comments:

The manuscripts 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 sen-

sitivity 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.

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).

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.

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.

Specific comments and technical corrections:

1)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.

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

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.

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

5)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.

6)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.

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

snowmelt on k?

8)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?

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

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?

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

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 3315, 2014.