Hydrol. Earth Syst. Sci. Discuss., 11, C1627–C1629, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C1627/2014/

Hydrology and Earth System Sciences
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.

M. Safeeq et al.

Mohammad.Safeeq@oregonstate.edu

Received and published: 31 May 2014

We thank this reviewer (Anonymous Referee #1) for providing detailed comments on our discussion paper. Below are responses (R) 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 Q0 (equation 6), which in turn is dependent on rainfall, snowmelt, and ET (equation 2). Based on equation 2, it is fair to assume that Q0 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.

(R1)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 Q0 (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 Q0 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 Q0 along with k are "intrinsic hydrogeologic properties" and unique to each landscape.

(2) For the purpose of mapping the streamflow sensitivity metrics (Figure 8), Q0 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 Q0 with IR or IM in their conceptual model.

(R2)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. 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 will discuss limitations of our approach.

(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. (R3)We will make the suggested changes in the final revision. (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 R2 values of 0.50 to 0.59 as "reasonable accuracy". Why not just state the R2 values and let the reader be the judge of accuracy? (R4)A similar concern was also raised by Referee #3. We will make the suggested changes in the final revision. (5) Was model validation (Section 5) done at all 227 catchments? If yes, please state it explicitly in that section. (R5)We have used 217 watersheds for validation (Fig 1). We will make the suggested changes in the final revision. (6) P3330, L23: Please change 'ragne' to 'range'. (R6)We will make 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. (R7)That is an excellent point and we will make the suggested changes in the final revision.

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