Hydrol. Earth Syst. Sci. Discuss., 11, C932–C934, 2014
www.hydrol-earth-syst-sci-discuss.net/11/C932/2014/

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.

Anonymous Referee #1

Received and published: 15 April 2014

This is an interesting article whose topic fits well within the remit of HESS journal. In my opinion, the novel contributions of the presented study are: (1) the regression model that has been used to map the baseflow recession coefficient (k) for the entire states of Oregon and Washington, and (2) mapping of the two streamflow sensitivity metrics for Oregon and Washington. All other analyses in the study are closely related to these two contributions. I recommend the publication of this article once my following concerns have been addressed:

(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. Sen-

sitivity 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.

(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 characterisation 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.

(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.

(4) P3326, L6: "Irrespective of geographic domain (OR, WA or both combined), it is

HESSD

11, C932–C934, 2014

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?

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

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

(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.

References

Wigington, P. J., Leibowitz, S. G., Comeleo, R. L. and Ebersole, J. L. (2013), Oregon Hydrologic Landscapes: A Classification Framework. JAWRA Journal of the American Water Resources Association, 49: 163–182. doi: 10.1111/jawr.12009.

Patil, S. D., Wigington, P. J., Leibowitz, S. G. and Comeleo, R. L. (2013), Use of Hydrologic Landscape Classification to Diagnose Streamflow Predictability in Oregon. JAWRA Journal of the American Water Resources Association, doi: 10.1111/jawr.12143.

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