"Snow Persistence Explains Stream High Flow and Low Flow Signatures with Differing Relationships by Aridity and Climatic Seasonality" by Le et al.

Response to Anonymous Referee #1 by Le et al.

Referee #1 comments:

Authors analysed how snow persistence, aridity, and seasonality conditions control streamflow, specifically flashiness at low and high flows in 1187 catchments across United States and Canada. They addressed two main research questions, which are 1) Can snow persistence explain the shape of streamflow hydrographs at low and high flow conditions? and 2) How do aridity and seasonality affect the ability of snow persistence to explaining shape-based signatures at stream low and high flow conditions? Authors found that for low flow conditions, larger snow persistence increases baseflow and reduces low flow variability regardless catchment aridity and seasonality. Authors also showed that snow persistence became a stronger factor in controlling baseflow, in regions with a relatively high aridity and/or with summer-dominant precipitation regimes. Based on analyses, authors concluded that snow persistence may serve as a useful streamflow descriptor across different climates and runoff regimes.

In my opinion, authors did an interesting work. I agree with authors, that the use of snow persistence as a metric describing catchment response across a wide range of climates is novel. Besides that, I see the novelty that this metric can be derived from satellite data and thus it can be used at a global scale. I also like that author related the snow persistence to different aridity and seasonality of catchments. Although the results are not much surprising as they mostly confirm our existing knowledge, I found the study important and novel, thus appropriate for HESS. However, I have several comments listed below, which should be addressed before I can recommend the manuscript for publication.

Major comments

After reading of abstract and study objectives, I was really motivated in further reading since I was curious about what authors investigated. However, I was a bit disappointed, because in my opinion, the study conclusions are not sufficiently supported by results and illustrations. In my opinion, only two figures showing study results (Fig. 2 and 3) are too few to draw general conclusions. Therefore, I would encourage authors to support their results with further analysis and figures. In comments below, I tried to make a few suggestions which authors may consider.

In my opinion, it might be interesting to look whether the snow persistence is a good predictor for the selected runoff signatures in years with snow-poor and snow-rich winters (or dry/wet, cold/warm years). Comparing statistics of individual years instead of mean statistics of the whole study period may allow for a more direct attribution of the inter-annual variations of snowpack to variations in runoff characteristics.

I see the evidence provided by authors that snow persistence can partly explain the selected streamflow signatures. However, the snow regime belongs to the main component of the water balance in high-

elevation and/or high-latitude catchments next to precipitation and its seasonality and evapotranspiration. Therefore, similar results would be maybe achieved for any of these characteristics. It means, that not only snow persistence might explain streamflow characteristics, but also aridity or seasonality indexes might bring similar results. Therefore, to further support existing results, it might be interesting to look how strong is the snow persistence as a predictor compared to aridity and seasonality indexes. Maybe, at least some correlation analysis comparing the predictive strength of all predictors might be beneficial. In addition to my above comments, results section should be extended. As it is now, it contains only a short description of results shown in Fig. 3 and it looks unproportionally short compared to the discussion section. As I mentioned above, interpretation based on one or two figures seems unconvincingly to me and I would encourage the authors to add more analysis and related interpretation which may further support (so far interesting) results.

Specific comments

L 105: I would somewhere mention the basic statistics of the study catchments (e.g., as a range of values), such as area, elevation, annual precipitation, snow persistence, etc.

L 110: I suggest including equations of how the main characteristics (snow persistence, aridity index, seasonality index) have been calculated.

L 147: Maybe I did not understand correctly, but 30% of area difference between the two different approaches of area calculation sounds as a large difference. Why is it so much?

L149: I understand that only perennial rivers were considered for the analysis. Nevertheless, would the results be different if also river intermittency would be considered? Please discuss shortly.

L173: 30^{th} and 70^{th} percentiles for low- and high FDCs sound rather as arbitrary choice. Is there any reason for choosing exactly those thresholds?

Authors defined several streamflow signatures for the analysis. This is fine, but I would suggest including a few more, for example low flow duration or deficit volumes. Especially the former might be beneficial to further explain the role of snow persistence on low flow regime.

L 195: How the last day with snow presence has been calculated? Due to elevation range of individual catchments, the snow may be melted at lower elevations while some snow may be still present at higher elevations. Please clarify shortly.

L286-287: The research questions here are repetition from above, please considerer whether it is needed to introduce them again at this place.

L320: Maybe I missed it somehow, but I do not see reducing low flow variability from Fig. 3a-c as noted by authors. Please, add some more explanation.

Technical corrections

L154-155: I would omit these two lines.

L296: “snowpack” rather than “snow pack”.

Fig. 4: Consider adjusting light blue and moderate blue colours in the figure since one can hardly see the difference between both.

L388: Perhaps, the brackets in “changes in” are not necessary.

L606: If I checked correctly, Muñoz-Sabater et al. (2021) has already the final paper published.

Response to Referee #1 Comments:

Response to Major Comments

We thank the reviewer for their helpful and positive comments.
In response to the major comments regarding the analyses, in our next revision of the manuscript, we will expand the set of analyses. First, as suggested by the reviewer, we will include additional analyses of high and low flow behaviour through the inclusion of high flow duration and low flow duration signatures. Our preliminary results show a strong relationship between snow persistence and both low and high flow durations, following the same directions as estimated for other low flow and high flow signatures used in the original manuscript.

Next, we believe that the suggestion to compare individual year statistics is an interesting direction to take this paper and we will explore correlational analyses of high snow persistent/low snow persistent years as well as across dry and wet years. We will then compare the correlational effects of aridity, seasonality, and snow persistence to further improve our analysis of the relationships between snow persistence and runoff behaviour in various climatic conditions. Through these additional analyses, we aim to provide a more direct attribution of the variations in snowpack to variations in runoff characteristics.

Response to Specific Comments

In response to the specific comments proposed by the reviewer, we will further clarify our study's methodology to help address these comments. We will also extract one additional figure from Fig. 3 to further illustrate and explain the interactive relationships between snow persistence, aridity index and seasonality index. This figure would show the regression lines between snow persistence and each signature at different levels (1^{st} quartile, median, and 3^{rd} quartile) of the aridity index and seasonality index. We believe this figure would clearly illustrate the impact of snow persistence on our shape-based streamflow signatures, which might be hard for readers to visualize from Fig. 3 alone.

Response to Technical corrections

We agree with the suggested technical corrections and will fix them in the revised manuscript.