Authors' response to interactive comment of the anonymous Referee #2

Black text: Referee comment

Blue text: Authors' response

Interactive comment on "Importance of maximum snow accumulation for summer low flows in humid catchments" by M. Jenicek et al.

I reviewed the paper "Importance of maximum snow accumulation for summer low flow in humid catchments" by Jenicek et al. Overall, I am quite intrigued by this topic, hence my reason for reviewing the paper. However, I found the paper lacking a clear takehome message and was often confused by the writing and organization of the paper.

The authors lay out two interesting research questions: 1) determine the length of memory effect on low flow conditions for maximum SWE and 2) estimate the sensitivity of catchments to changes in snowpack. While I find these questions compelling they don't seem to be very well answered by the study (see major comments below). In particular, the authors seem to neglect discussing the very high correlation between low flows and winter precipitation (almost always explaining more variability than maximum SWE). I think this may be an opportunity, rather than a limitation, to identify a novel response (see major comments). Shifting the study questions to better reflect the [potential] novelty of the work is needed. Secondly, I do not feel that the authors adequately address their second question about sensitivity to changing snowpacks. They seem to suggest that high elevation catchments are as much or more sensitive to warming as low elevation catchments. This seems like a large simplification (see major comments below). I lay out several potential ways to reframe the work that may help address its novelty.

We thank the reviewer for the many valuable comments and suggestions to improve our contribution. We agree, that most of findings are not surprising as they mostly support our qualitative existing knowledge of how snow contributes to summer runoff. However, we believe that the quantification of snow importance in selected area is a valuable and novel contribution and that the findings are still important also if they don't bring any change of our process understanding. Additionally, we benefit from recently generated SWE data sets which, in our opinion, improved presented analyses. Below, we tried to answer the major comments of the reviewer. Because we found several valuable comments in the reviewer's text, we used sub-headers to separate the individual issues.

Major comments:

Novelty of the work: To me the important questions for a climate like Switzerland are 1) does changes in the timing of snowmelt or changes from winter snow to rain alter summer baseflow? and 2) can summer precipitation counteract the effects of changing winter precipitation inputs? The paper currently feels like it is arguing that SWE is more important than precipitation (which is not supported by the results) and that SWE becomes more important during dry summers (which is really not surprising). From my perspective the real question is does SWE (timing or amount or S/P) explain additional variability in low flows beyond what winter precipitation explains. This may require a different analysis, possibly normalizing for winter precipitation or some type of step-wise regression. One might hypothesize that snowpacks release water later in the year, so the timing of snow disappearance may be the critical information (in addition to winter precip amount). Currently, the paper suffers greatly by not discussing that winter precipitation explains as much or more variance of

low flows than SWE variables (Table 3). I also suggest that the authors use wet and dry summers to ask when summer precipitation can overcome poor snowpacks or dry winters. Perhaps this could be accomplished using an elasticity type relationship for both summer and winter precipitation and SWE. Do you need more summer precipitation to drive the same low flows that winter precipitation (i.e. winter precipitation is more efficiently partitioned to streamflow)? This is an important question that has large climate change impacts. Along those same lines, I strongly encourage the authors to move away from their second research question about sensitivity to changing snowpacks unless they significantly bolster related analyses (see comment below). This is a great discussion point, but currently poorly addressed.

Research questions

We thank the reviewer for suggestions to modify the research questions. We believe that there are still many interesting questions and issues related to the topic which are currently not fully answered and should be definitely investigated in the future. The drought occurred in central Europe this year is a good reason for further research of this topic. For additional explanation, see also part named "Catchment sensitivity".

Combined effect of snow and precipitation

We do not intend to argue that SWE is more important than precipitation (this is really not supported by our results). Due to moderate humid climate in Switzerland with precipitation almost equally distributed in a year (opposite to western US), the aim was to show the combined effect of snow and liquid precipitation and their changing role in time (in different months) and in catchments with different elevation. Additionally, we wanted to quantify the effect of snow on minimum discharges when liquid precipitation is below average (or opposite, when SWE is below average) as documented in Fig. 7. This could increase the reliability of predictions of minimum discharge during summer. However, we could make these points clearer in the text, thus we will carefully address their clarity and comprehensibility in the revised version.

Timing of snow disappearance

We agree that timing of snow disappearance in specific catchment represents critical information (as also mentioned by the referee #1). So we will do a new analysis, focusing on the "memory effect" of individual catchments. In the present version, the memory effect could be seen in Fig 4. However, it doesn't account for different melt-out days in a specific catchment. We will use a new SWE data set and calculate melt-out days. By this, we will be able to compare the memory effect length to how long it takes to melt out the available snow.

Wet and dry years

Thank you for your suggestions regarding using wet and dry years. Although we already differentiated between years with below/above average precipitation (SWE) we will consider further analysis of this issue.

Winter precipitation

We used winter precipitation as a predictor and we expected similar results as with maximum SWE. Winter precipitation (from November to April) is highly correlated with SWE and we expect increasing mutual correlation for higher elevation catchments with higher S/P. Despite higher correlations (Table 3), we consider winter precipitation to be less suitable as a predictor than maximum SWE. The reason for this is that winter precipitation is not corrected for undercatch of snowfall. Thus, we expect larger errors varying between stations according to site conditions and wind speed. However, we agree that this issue is not discussed in the current version of manuscript and we will carefully address this in revised version.

Climate change effects are oversimplified: The authors use elevation as a means to organize the catchments and their sensitivity to SWE. This seems problematic given that there is generally a large

gradient of precipitation and S/P ratios across elevation. The discussion seems to imply that high elevation catchments are as much or more sensitive as low elevation catchments. This may be true if catchments are all near zero degrees and precipitation is evenly distributed across the winter, however, this is not discussed. I can imagine situations where high elevation catchments are less sensitive to a given amount of warming because they are well below 0 C for most of the winter. Given that the authors do not partition variance well between winter precipitation and SWE effects on low flows, I think the discussion of climate change is very weak. It is quite possible that the points suggested above may improve discussion points here, however, I suggest the authors do not make that a central research question but a discussion point (or significantly bolster the associated analyses).

Discussion of climate change

The insufficient discussion of climate change effect was mentioned by the other reviewer as well. We will carefully address this recommendation in the revised version of manuscript.

Catchment sensitivity

We do not argue that higher elevation catchments are more sensitive to low flow occurrence in all circumstances. Based on our results we can only quantify the potential decrease of minimum discharges in case of decrease of maximum SWE. We did not explore relations between possible warming in cold season and minimum discharges in warm season (although some indirect evidence of this could be found in Table 3 using predictor named "sum of positive air temperature"). We do not know if this SWE decrease will occur. However if it happens, than the same percentage SWE decrease in higher elevation catchments will results in stronger percentage decrease of minimum discharges (see Fig. 5 showing the elasticity). We agree, that this should be better clarified in the text and we will carefully address this in revised version. Additionally, different sensitivity to drought in catchments with different elevations was also described by Staudinger et al. (2015) who made similar conclusions. All above mentioned circumstances led us to establish the research questions regarding catchment sensitivity which we believe to be valuable. However, we are aware that our results could only partly answer these questions and we think this needs to be investigated in the following research.

Use of monthly/weekly low flows: I am mixed about the use of monthly/weekly low flows. On one hand, this fits with the question about memory effects on low flows that the authors pose. It also gets around potential issues with noisy annual low flow data. On the other hand, what is a low flow in May and why does anyone care? I find the use of the lowest summer flow as a much more compelling response variable to predict. The use of monthly low flows is particularly problematic early in the summer when some watersheds are storing water as snow and others are not. In some ways the current effort is quantifying the recession relationship of the watersheds, which is [in my opinion] not the focus of the paper. Perhaps I am missing something here that could be better explained in the text.

Monthly/weekly low flows

Clearly we see the lowest summer flow as a compelling response variable, given the water management interest and possible issues connected to it. However, for the development of the role of snow compared to liquid precipitation this one response variable is not sufficient. We agree that the mixing of monthly/weekly data could lead to some confusion. We think that monthly approach is sufficient for most analysis we did. However, for memory effect calculation we used weekly data which enable to see slowly decreasing effect of snow in minimum discharges (see Fig. 4). Additionally (as discussed above), we will include melt-out days to improve the interpretation of catchment's memory effect.

Starting day of low flow analysis

We choose period from May to September to show the changing importance of snow contribution to low flows in different catchments, both in lower and higher elevations (melt-out occurs usually in early April in lower elevation catchments of our selection). We believe that this is helpful especially when looking on Fig 2 (bottom line) and Fig 4. We also tested the effect of snow on summer minimum discharge (June-August, not shown in the current version of manuscript). The results for most of catchments are very similar to existing relations calculated for August as most of summer (Jun-Aug) minimum discharges occur in August. This was also the reason we choose monthly/weekly step. This information will be added to the text. Additionally, we will consider if it is possible (and informative) to present results also for the lowest summer flow.

Minor comments:

Abstract has no quantitative results

We will consider modification of abstract in order to be more specific.

Introduction seems to wander from idea to idea without a clear structure. Too many paragraphs that talk about similar ideas.

When writing the Introduction, we followed sequence "changes of SWE/winter precipitation (mainly due to climate change) – consequences to runoff changes – liability/sensitivity of different regions to described changes in snowpack". We will carefully consider modification of the text in order to be clearer.

I would like to see a table of the mean and CV of all predictor variables and response variables.

We agree, this table could bring valuable information. We will provide new table or modify Table 2 where this information could be also placed. As for mean values of maximum SWE and S/P, this information is now in Table 1.

The figures are extremely hard to read in black and white, which many people will do when printed. Particularly Figure 4, 7, and 8.

We agree with this. However, the use of colors enables to provide the reader with additional helpful information (e.g. catchment elevation as it is in Fig. 8). Nevertheless, we will consider possible changes in order to use colors that are distinctive when printing in greyscale. At least we will mention the colors better in the figure caption so that readers will realize that they should look at the color version (actually these days many readers will not print papers but read the pdfs at a computer or tablet, where color makes figures more readable).

References

Staudinger M, Weiler M, Seibert J (2015) Quantifying sensitivity to droughts – an experimental modeling approach. Hydrol Earth Syst Sci 19:1371–1384. doi: 10.5194/hess-19-1371-2015