Authors' response to interactive comment of the anonymous Referee #1

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.

Jenicek and colleagues present a data driven study, which uses climate and runoff data to study the effect of snow storage and precipitation on summer low flows for 14 catchments in Switzerland. The main findings of the paper are (i) maximum winter snow accumulation influenced summer low flow, but is not the only controlling factor, (ii) in years with below average precipitation amounts during spring and summer the importance of snow accumulation increased, (iii) the sensitivity of summer low flow to snow accumulation is higher in high elevation catchments. Although understanding the role of (changing) snow conditions on summer low flows is a relevant topic for HESS I do have some serious concerns about the current version of the paper, and the papers needs to be significantly improved before this paper can be considered for publication in HESS:

1. I am not sure if the findings of the paper are significantly novel, or if they provide useful new insight in the role of snow for summer low flow conditions, because the analysis only relies on statistical relationships between snow conditions and summer low flow conditions but does not provide any mechanistic explanations of the relationships you describe. The statistical findings by itself (in my opinion) only confirm some obvious qualitative findings that are not surprising: the fact that (i) maximum winter snow accumulation influenced summer low flow, but is not the only controlling factor is not surprising, and differences with for example Godsey et al. [2014] are not really surprising either if you consider the strong Winter dominated precipitation regime of the Western US compared to more constant (and even sometimes summer dominated) precipitation regimes of Switzerland. Also finding (ii) in years with below average precipitation amounts during spring and summer the importance of snow accumulation increased only seems obvious to me, similar to (iii) the sensitivity of summer low flow to snow accumulation is higher in high elevation catchments because in these catchments snow is a higher fraction of the total water balance of the catchment (compared to rain) thus a % change in snow is likely to lead to a larger % in runoff (if all other factors are the same). I do not argue that results of empirical analyses are only valuable if they confirm some unexpected, but I do think there is some novelty lacking in this paper as I don't see how the paper really provides new understanding, refined previous understanding, or helps with better prediction of summer low flow conditions in Switzerland. Thus, I would recommend the author's to (i) either write the manuscript such that novel contributions are better highlighted where you show how we really improved our understanding of ability to predict, or come up with some additional analyses that would allow this.

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 existing qualitative knowledge of how snow contributes to summer runoff. However, we believe that the quantification of snow importance is a valuable and novel contribution. We also argue that the findings are still important even if they do not change our process understanding. Additionally, results of this study indicated regions which might became more vulnerable to drought occurrence in the future because of decrease of snow accumulations and snowfall fraction during cold period. We

benefit from recently generated SWE data sets which, in our opinion, significantly improved presented analyses.

The referee suggests to "(*i) either write the manuscript such that novel contributions are better highlighted, or (ii) come up with some additional analyses that would allow this*." We are planning to combine both of mentioned recommendations in the revised version of manuscript as outlined in following points:

- We will do additional analyses focusing on "memory effect" of individual catchments. In present version of manuscript, the memory effect could be seen in Fig 4. However, it doesn't account for different melt-out day in specific catchment. In the revised version of the paper, we will use a new SWE data set and calculate melt-out days. Doing this, we will be able to better compare the memory effect length to how long it takes to melt out the available snow.
- The role of catchment properties was analyzed and summarized in Table 4. Although, we looked on the role of selected catchment properties on catchment sensitivity (using Theil-Sen regression slopes), this part was not the main goal of the study. However, we will focus more on catchment properties and any valuable results will be added to the revised version. See also answer to major comment 3.
- We agree that some parts of the article have to be rewritten in order to better highlight most important results arising from our study. We will carefully address this in revised version of manuscript.

2. You choose at set of 8 predictors for summer low flow conditions. Maybe the choice of indicators is obvious for you, but clarify why you chose them.

   We agree, that the explanation is missing in the current version of paper. We will clarify this in the methodology part of revised version. The advantage of this choice is that only SWE, P, T and Q data are needed. These data are usually available also for other regions which enables to prove or disprove our results also in other world's regions with possible transfer to ungauged catchments.

3. You state that "maximum winter snow accumulation influenced summer low flow, but could only partly explain the observed inter-annual variations. One other important factor was the precipitation between maximum snow accumulation and summer low flow". Although I agree with statement, I think the manuscript lacks a more thorough discussion of other factors that can explain low flow conditions. As an example, what about evaporation differences between years? They are a major component of you catchment's water budget, affect water storage (and thus low flow conditions), but are completely unmentioned. Or what about the role of landscape draining properties (e.g. Tague et a., [2004])

   We agree that our results do not show the process causality. It means we are not able to precisely explain possible reasons of relations shown in our results. We just explored the dependences and quantified them for different catchments. It means, we are able to estimate how sensitive the catchments are to any change of winter snowpack but the reasons still remains unclear and should be further investigated in the future. Still, we give partial explanation related to catchment properties, namely elevation, size, slope, S/P and maximum SWE (Table 4). However, we agree that this issue should be better addressed in the paper. Thus, we will focus more on catchment properties in discussion part.

   Evapotranspiration (both potential and actual) was not included because of lack of available data. Water balance component estimates for the entire of Switzerland during the last 100 years show that annual precipitation and runoff vary far more than evaporation (Hubacher R., Schädler B., 2010). So we expect that variations of ET from year to year are relatively minor and if there are any, than we would expect them to be just opposite, i.e. more AET in wet years and

less in dry ones. This feedback is another reason why we think AET is less useful as predictor. However we agree that this issue should be discussed in the revised version of manuscript.

4. The analysis is based on catchment average values and the catchment divided into two parts. Is this strongly limiting your analyses for a catchment larger than 1500km^2?

   We are aware that using catchment means in catchments with different size can make the interpretability more difficult. However, we decided to do the analysis for entire catchment scale (but not for large basins). Thus, we used both SWE calculated as a catchment mean and SWE calculated from higher situated 50% of catchment area. We assumed, that snow in higher elevation could be more important for summer low flows. However, both approaches brought nearly identical results. Additionally, Spearman rank correlations are not significant when looking on the effect of catchment size on catchment sensitivity to SWE decrease (Table 4). On the contrary, we cannot exclude the effect of size in very large basins (except Hinterrhein, all studied catchments are smaller than 800 km^2). Including larger catchments is not feasible, as the larger catchments in Switzerland are significantly affected by hydropower regulation.

5. I have difficulty to efficiently read the results section. The section refers to the graphs and tables but does not explicitly takes the reader by the hand in explaining what part of the graph we should focus on when you conclude anything from these graphs.

   Thank you for this comment. We will reformulate our result section to be clearer and more helpful to the reader.

Technical comments

Abstract Line 3: It isn't really "winter" precipitation that is sensitive to temperature changes, which implies a 3-month season, but rather something like "cold season".

We agree. We will change "winter" to "cold season".

Lines 3-4: "snow" does incorporate both "snowfall" and "snow storage"?

We agree. The "snow storage" is a better expression in this case.

Line 4-5: Does it necessarily relates to "groundwater" recharge as water in some catchments may mostly only reach the unsaturated zone?

We understand the point, although this is only general information. However, we will change it to "...will affect soil and groundwater storages" to be clearer.

Line 8: Instead of "snow", be specific if you mean "snowfall", "snowpack" or both.

We agree. We will change "snow" to "snow storage".

Line 21: since you haven't defined the elasticity index it is difficult to interpret your statement by just reading the abstract.

We believe, the elasticity should be generally known as it is often used in many studies (mainly climate studies). However, we will consider reformulation of the sentence.

Introduction (7025), Line 2-3: Is "The shift from snowfall to rain" one of the most important effects of predicted climate change "in general" (as you currently state) or "on the hydrological cycle".

We agree. We will add "..on the hydrological cycle" to be clearer.

Line 9: The reference of Berghuijs et al [2014] studies inter-annual and mean-annual water balances and only speculate what the changes in seasonal hydrology could be. Hence the reference is not really appropriate here. Also, please be specific with what you mean by "might influence"; e.g. modelling results indicated that ....

We agree. We will reformulate the sentence.

Line 10-21: There are clear differences between the findings of the reduced snow days in Switzerland (mainly in spring) and western US (mainly in Winter). It might be good to highlight that more explicitly.

We assume that the reviewer means the ratio of snowfall days and precipitation days (SD/PD). Mentioned differences between catchments in western US and Switzerland (spring vs. winter decrease) seems to be simply due to different mean air temperatures in both regions (generally lower winter air temperature in Swiss catchments and thus later effect of changing SD/PD). We will highlight it more explicitly in the text.

Introduction (7026), Lines 8 - 12: Since studies find regional differences in the streamflow trends, do they also have different physical explanations, and are these explanations relevant to mention?

Studies mentioned in this paragraph were selected just to document regional differences in streamflow trends. As some studies also tried to explain differences within a studied region (Birsan et al. 2005; Fiala et al. 2010), physical explanation for differences between regions is not clear (could be e.g. due to increasing continentality from west to east direction in Europe). However, we think that this simple information about streamflow trends in different European regions is still valuable here.

Line 15: “above 1000ma.s.l. and below 2500" or “between 1500 and 2500 m.a.s.l"?

We agree. We will reformulate it (“between 1500 and 2500 m a.s.l”).

Line 16: Can you be more specific than “more sensitive"? Was it a large or small difference? Does this still focus on mean runoff?

We agree. We will add an explanation and quantification.

Line 26-28: “However, the ... al., 2015)." Rewrite the sentence such that it reads well and that it is clear if you made up a statement yourself or it is based on a reference.

We agree. We will reformulate the sentence.

Introduction (7027), Line 3: Specify this is the Sierra Nevada in the US (and not Spain, or Colombia).

We agree. We will add “in the US”.

Line 7: Unclear what a “longer memory effect" exactly means. It is important to make this clear as this is also mentioned in your objectives of the study.

We agree. We will provide an explanation.

Line 16: what about spring precipitation?

We agree. We will change “spring precipitation” to “liquid spring and summer precipitation”.

Line 17: I do not see how you look at their spatial influence. Do you mean between catchment differences?

Yes, with spatial influence we mean the differences between investigated catchments in different elevations. We will reformulate that.

Lines 18-19: I don’t think this statement is very clear “To explore ... amounts overall". Why is this more important here?

With this sentence we wanted to highlight the fact that majority of studies focusing on the effect of snow on summer minimum discharges have been made in regions with different precipitation patterns. The example is western US which is the region with different precipitation seasonality and lower total annual precipitation compared to Switzerland (humid climate where annual precipitation are more equally distributed during year and their total amount is considerably higher in some cases). We will reformulate for clarity.

Study area, Lines 25-26: What do you mean by “as close as possible to natural conditions". Does this mean there are no land-use changes? Does it only refer to dams in the river?

We mean that streamflow is near-natural and no major human influences by dams, water transfer etc. are present. We will clarify this in the revised version.

Page 7028, Line 22-24: Be more specific in what Jorg-Hess et al. (2014) already did regarding the link of SWE and low flows.

We agree. We will add an explanation.

Page 7029, Line 12: Clarify why you chose this set of predictors. Maybe it is very obvious, but you currently do not explain your choice

See answer to major comment 2. The reasons of choice of the set of predictors will be described in revised version of manuscript.

Line 26: why did you set the threshold at 1.1C? Is this based on another study? Does the threshold affect your choice?

The threshold temperature near 1°C is used by several authors (Dai 2008; Feiccabrino and Lundberg 2008) who usually used data from stations where the information about phase of precipitation is available. Additionally, we tested different temperatures, and found no sensitivity of exact value on our results. We will add this information as well as literature references to revised version of manuscript.

Page 7031, Line 2: What do you exactly mean by “is more obvious"

Thank you for this notice, the current sentence is not fully clear. We will reformulate it as follows: “... and thus dependent variable (e.g. minimum discharge) is more sensitive to the change of the independent variable (e.g. maximum SWE).

Page 7032: Section 3.1: be explicit which results of the table you use to make these conclusions.

We agree. Please, see the answer to major comment 5.

Page 7034: Section 3.3 Explain why you use these three catchments?

We selected three snow-dominated catchments in high and middle elevations (as mentioned in the text) in order to show the effect of combined effect of snow and liquid precipitation. These catchments were selected as typical representatives. Although, there are some differences between all studied catchments in high and middle elevations, all of them show similar behavior. Therefore, to reduce the manuscript extent, we decided to show only three representatives. We will add a short explanation in the text.

Discussion section: the discussion of the sensitivity to climate change is really short and does not include any effects different expected SWE changes between the different altitudes. In lower elevation catchments the % of SWE is much more sensitive to temperature changes, than high elevation catchments. This needs to be emphasized. Also, a statement as “This reduction might increase problems with water availability in affected regions" is not really helpful if you do not provide any numbers of changes you expect with for example a 2 degrees warming.

This point was mentioned also by second reviewer. We will consider this notice in the revised version of manuscript.

I don’t learn anything from the the discussion section on “Combined effect of snow and precipitation".

This paragraph aimed to highlight that liquid precipitation could only partly overlay the effect of snow. Even in case of high liquid precipitation, the minimum discharge remains lower in case of low

snow conditions in previous winter. However, we will consider reformulation of this paragraph to better address this issue.

Maybe to my ignorance but I do not understand the argument of using SWE since you don't have groundwater data "Snow melt ... minimum discharge (Fig. 2)"

We agree. This sentence is not necessary in the result section and will be removed.

7039, Line 11: Be more specific than "significantly affected low flows". Does it change the volume of low flows, the timing of low flows or both?

This point summarized the different period by which snow contributes to minimum discharge in catchments with different elevation (it means only timing is considered in this case). We will reformulate the sentence to be more clear and specific.

It is unclear if the statement "Low flows occurred later in the year for years with above average snow accumulations. A decrease of maximum snow accumulations by 100mm resulted in earlier runoff minima by 12 days" is applicable for all catchment?

This statement is valid for all selected catchments (on average) considering their mean day of minimum discharge occurrence. We will reformulate the sentence to be clearer.

Line 20-24: "Snow and ... were considered." It is unclear if the combination of rain and snowpack can sufficiently explain low flow conditions or if more information is needed.

We agree that the formulation is not fully clear. Even if consider both snow and liquid precipitation, there is still some portion of annual variability which cannot be explained by these two predictors. We will reformulate the sentence to be clearer.

Figure 1: Does it make sense to also have the altitude differences in Switzerland indicated on the map?

This was also suggested in the short comment posted by Michael Stölzle. We added DTM as an additional information into Fig. 1.

Figure 2: I doubt this will be readable when printed on a A4 format.

It should be readable. However we will enlarge the figure labels.

Figure 4: How do you explain the significant negative correlations?

We do not see any physical explanation for negative correlation and we consider them as a noise. Additionally, most of them are not statistically significant (0.05 level). This information will be added to the text.

Figure 6: Make clear what the reference date is on the y-axis.

The day of year "1" represents the first day of calendar year (1.1) and day of year "365" represents 31.12. This information will be added to the figure caption.

Figure 8: Are the labels of this figure readable when printed on A4 format? Can you provide a color-scale for the elevation indication?

It should be readable. However we agree readability would be improved with larger labels. As for color-scale, the information was put in the figure caption in current version of text (and it is same as in Fig 5 and 6). However, we will provide the color-scale directly to the figure.

References

Birsan, M. V., Molnar, P., Burlando, P. and Pfaundler, M.: Streamflow trends in Switzerland, J. Hydrol., 314, 312–329, doi:10.1016/j.jhydrol.2005.06.008, 2005.

Dai, A.: Temperature and pressure dependence of the rain-snow phase transition over land and ocean, Geophys. Res. Lett., 35(12), L12802, doi:10.1029/2008GL033295, 2008.

Feiccabrino, J. and Lundberg, A.: Precipitation Phase Discrimination in Sweden, in 65th Eastern Snow Conference, pp. 239–254., 2008.

Fiala, T., Ouarda, T. B. M. J. and Hladný, J.: Evolution of low flows in the Czech Republic, J. Hydrol., 393, 206–218, doi:10.1016/j.jhydrol.2010.08.018, 2010.

Hubacher R., Schädler B.: Wasserhaushalt grosser Einzugsgebiete im 20. Jahrhundert. Tafel 6.6. In: Weingartner R., Spreafico M. (Hrsg.): Hydrologischer Atlas der Schweiz (HADES). Bundesamt für Umwelt, Bern, 2010.