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

Black text: Referee comment

Blue text: Authors’ response

We thank the reviewer for the valuable comments and suggestions to improve our contribution. We provide point-by-point reply below.

The paper presents how snow processes influence runoff generation in mountainous catchments in Czechia. The presented results are not novel, and similar things have been shown across different regions. However, the manuscript could still be a valuable contribution for the readership of HESS. The overall structure of the manuscript is quite clear, but inconsistent language makes the paper sometimes hard to follow, especially throughout the introduction and discussion. Below I suggest some changes that should be considered prior to publication.

At this point, I am not convinced by the conclusion that “snow is more effective in generating catchment runoff compared to liquid precipitation“. First of all, it is not clear what I actually see in Figure 3: Did you plot the mean of both groups (snow rich and snow poor) for every catchment? Please add some information to make this clearer.

Thank you for the comment. As you correctly assume, individual points in Figure 3 represent mean snowfall fractions and snow runoff fractions for snow-poor and snow-rich years for individual catchments. The snow-rich years were defined as years with annual SWE_{max} above the third quartile of the study period (1980-2014), while the snow-poor years represent years with annual SWE_{max} below the first quartile of the study period. Therefore, each point represents a mean calculated from 8-9 annual values derived from ~35-year-long time series. We will add more explanation to the figure caption to be clearer.

With Figure 3, we wanted to quantify to what degree the snowfall is important for runoff generation compared to rainfall in our study catchments. This was also assessed by runoff coefficients calculated separately for snowfall to snowmelt runoff and rain to rainfall runoff (values are not shown in the paper, but they are discussed in Section 3.2). The higher runoff fraction for snow-generated runoff is caused mainly by lower actual evapotranspiration during winter (see also our response to Referee 1). Additionally, in modelling experiments we showed that the transition of snowfall to rain caused changes in 1) annual water balance and 2) seasonal runoff distribution, affecting groundwater recharge and summer low flows (Fig. 8). We will add more explanation to the methods Section 2.4 (regarding snow runoff calculation in HBV), to results Section 3.2 (better description of Figure 3) and also to the related part of the discussion section (consequences for seasonal runoff distribution and water availability).

Second, I’d like to see the same calculations (Figure 3) with the absolute values for total snowmelt runoff and total snowfall precipitation as 26% (on average) of total runoff might still be less than 20% (on average) precipitation.

You are right that, in absolute values, snowfall represents a higher water volume than runoff from snow. By definition of the “effect tracking” algorithm in HBV (as described in Section 2.4), the source of the input water (precipitation) can be changed only by refreezing (from rain to snow), but this process is rather negligible in absolute terms. Therefore, snowfall will be always higher than snowmelt runoff over the defined longer period (losses from evapotranspiration will likely be always higher than changes of water source from rain to snow due to refreezing). Nevertheless, in few catchments it happened that few hydrological years showed higher snowmelt runoff than snowfall in that year since part of this snowmelt contribution comes from previous year (probably thanks to long catchment storage). The mention effect is certainly worth for further investigation, but it goes beyond the scope of the current manuscript.

We carefully considered your suggestion to make a new figure with absolute snowfall and snowmelt runoff values, but we prefer not to include it since we think it would not provide any new information. Nevertheless, we will add more text regarding this issue to the methods (Section 2.4) and to the discussion to provide the reader with thorough explanation and interpretation.

Also the increasing trend with elevation in my opinion is not visible in the results. There needs to be further analysis (maybe cluster in elevation groups) to convince readers. I understand that some of these results are also supported by the HBV modelling. However, you need to more explicitly convince readers that snow vs. rainfall processes can be well separated in the current modelling setup.

You are right that we cannot make a direct conclusion regarding the effect of elevation. The increase in the difference between snowfall fraction and snow runoff fraction is statistically significant for catchments with higher snowfall fraction (see Fig. 3b) rather than elevation. In general, the elevation dependence is not directly evident from results, although some other results in our study (e.g. Fig. 5) indicated the relation with snowfall fraction, which generally increases with mean catchment elevation. However, the correlation of snowfall fraction and mean catchment elevation is not high, although significant (Pearson correlation coefficient is 0.53, p-value<0.001). The relatively lower correlation might be caused by the fact that mean catchment elevation cannot describe the hypsography of individual catchments and also by the fact that the variability of the mean catchment elevations is not high (800 m between the lowest and the highest catchment). We will clarify it better in the revised version of the manuscript (both results and conclusion sections). We will also add some more discussion on this topic.

A better characterization of the catchments (i.e., the runoff regimes, precipitation and runoff seasonality) is warranted. This will help to better emphasize why these results are valuable and why it might be useful to show the results for these specific study regions. To people who are not familiar with topography and hydroclimatology of Czechia it would be very helpful to have more "background" information on the study catchments. Please add a table with information on mean, max, min size, elevation, precipitation, temperature, discharge,: : :

Thank you for the suggestion. We will add a table showing the main catchment attributes and meteorological characteristics. We agree that this information might be useful for readers. Similar comment was also made by Referee 1. Additionally, we will put also more emphasis on regional differences of results as mentioned in one of your detailed comments bellow.

What are the main differences between the regions, and the four sample catchments? This is important to interpret the results afterwards (some of them are shown based on the different sample catchments). If I interpret the DEM correctly your highest peak is only 1602 m a.s.l., some of the catchments are far below 1000m in peak elevation, do they even have snowfall / accumulation every year? I find it difficult that, in the discussion section, you interpret the results based on the different regions, however they are not well characterized.

The four selected catchments represent different geographical regions and elevations. We will describe them in more detail in the revised version. For this, the new table with catchments characteristics (as mentioned in previous comment) might help as well.

Although the mean catchment elevation ranges only from 491 to 1297 m a.s.l, all catchments have the seasonal snowpack every year (mean SWE_{max} for individual catchments ranges from 35 mm for lowest catchments to 664 mm for highest catchments). However, we agree that these regional differences are not well described both in results and discussion sections. We will add more details and interpretation to the revised version of the manuscript.

Detailed comments

line 98 you claim that the selection criterion is timeseries >35 years however in line 104 /105 you write that three catchments do have less data

It is correct that three catchments have shorter time series (by one or two years compared to the rest of catchments). We are aware that it may bring some inhomogeneity into results, but since the shortening is only one or two years, we decided to include those relatively snow-rich catchments to the analysis. We will describe it more clearly in the revised version of the manuscript.

line 125 although I tend to believe that annual precipitation, peak SWE did not change significantly it would be great to see this (maybe in a table in the supplementary)

Thank you for the comment. The mean SWE_{max} was 141 mm for the calibration period and 140 mm for the validation period; annual precipitation was 1104 mm for the calibration period and 1143 mm for the validation period. We will add those numbers to the respective paragraph in methods next to the information about the increase in mean annual temperature by 0.7°C between both periods.

line 155 what is the range of threshold temperature throughout the catchments?

Threshold temperatures for individual catchments arising from median simulations (100 parameter sets) ranges from -1.58°C to 1.13°C. We will add this information to the respective part of the methods section.

Section 3.1 is not overly informative, in my opinion it can be moved to the supplement.

We think that showing the results for calibration and validation might be important for many readers to assess the overall ability of the model to simulate the individual components of the water cycle. Putting this part into supplement would probably cause a lot of readers to simply miss the information especially if this would be the only supplementary information. Therefore, we prefer to keep this part in the main text (unless there will be need for even more supplements).

Figure 4 (and Figure 8): catchments are sorted by "mean" elevation, also add an arrow and write elevation next to y axis, and at least give starting and end value (115m a.s.l. to 1602m a.s.l.)

We agree, we will indicate the elevation ranges in Figure 4 (and 8) and add "mean" into the figure caption.

Figure 4 (and Figure 6): make it clear, that you show the results for four specific catchments maybe by using the catchment names as headlines for the subpanels)

We agree, adding the catchments names to individual panels may increase the readability of both figures.

Figure 5 and Figure 7: make sure that you use different color coding, as you show different things (in Figure 5 Sf and in Figure 5 the regions)

Thank you for the suggestion. We will change the colour coding in Figure 7 to avoid confusions with Figure 5.

Figure 5 please mention the abbreviations (as in the axis titles) also in the figure caption

We agree, we will add the abbreviations to the figure caption.

Figure 7 is a bit confusing: In panel (a), do you show a point for each catchment where x is the mean of baseflow from all years having below average summer precipitation and y is the mean of baseflow from all years having below average SWEmax? If that is what I see in Figure 7a, than 58 out of 59 catchments have below average summer baseflow when they experience below average summer precipitation. However, only 40 out of 59 catchments had lower summer baseflow when having lower SWE, which is not supporting your conclusion on the importance of SWE. Please revise this figure (and its caption) to make it clear what is shown.

With Figure 7, we wanted to show the relative importance of annual SWE_{max} and summer precipitation on summer baseflow. For example, Figure 7a shows the median summer baseflow relative anomalies for years with below average summer precipitation (x axis) compared to the median of summer baseflow relative anomalies for years with below average SWE_{max} (y axis). From Figure 7a it is clear that summer precipitation is more important for summer baseflow than SWE_{max} (as we mentioned in line 277 of the original manuscript). Nevertheless, Figure 7b indicated that for the majority of catchments, the summer baseflow for years with below-average summer precipitation increased when there was simultaneously above-average SWE_{max}. Moreover, some of the catchments showed even positive summer baseflow anomalies for above-average SWE_{max} despite negative anomaly of the summer precipitation. We are not saying that snow storages play a major role in generating summer baseflow, but results indicated that SWE is an important additional driver. An important implication for the future climate is that the summer baseflow might be lower because of lower snow storages even when summer precipitation would not change.

We agree that our explanation may not be fully clear. Therefore, we will consider reformulation (including changing the figure caption to be clearer).

In Figure 8 please consider using the same scale for the color bars to make the panels comparable.

Thank you for the suggestion. We considered using the same scale already during the manuscript preparation and decided in favour of different scales since the scales are of different magnitude. This is especially valid for panel (d) where the magnitude is of different order compared to other panels. However, we will try to standardize the scale for panels (a), (b) and (c), at least.

Discussion: You mention data errors in the headline of 4.1 but you did not discuss them.

Thank you for the notice. We will change the respective title to “HBV model setup and parameter uncertainty” to better describe the section content.

You need to better emphasize the challenges when separating liquid from solid precipitation within the HBV modelling framework. Maybe you can discuss the implications on your results a little more detailed.

The uncertainty of model parameters is discussed in the Section 4.1. We think it is an important topic since many of the model parameters might have an important effect on result interpretation. Nevertheless, a more detailed discussion of implications resulting from HBV parameterization was suggested also by Referee 1. Therefore, we will add more discussion on this topic.

The contribution from groundwater calculated with HBV is quite uncertain, you could also be looking at generally higher storage potential at higher elevations. Maybe you could consider discussing these uncertainties.

This comment also touches the issue mentioned by Referee 1. It is true that absolute values of groundwater storages simulated by the model may be uncertain since groundwater data were not used to calibrate the model (see also our response to Referee 1). Nevertheless, we were concentrated on relative differences (year-to-year variations) between groundwater fluxes in individual catchments rather than on absolute values. Some studies also showed that catchment storage calculated using different methods (water balance calculations, recession curve analysis, HBV modelling) are, in general, comparable and correlated, although the quantitative estimates may differ (Staudinger et al. 2017). The above study also showed that dynamic groundwater storage is correlated with elevation, indicating the relation of the groundwater storages and snow storages. The relative fraction of groundwater in streams and its sensitivity to inter-annual variations in snow storages was also shown by Carroll et al. (2019). Therefore, we will include more discussion on this topic to the revised version.

You mention a lot of interesting differences between the regions / catchments in the discussion, maybe you can add more information at an earlier part of the manuscript and build your story on these different regions.

Thank you for the suggestion. We agree that we should put more emphasis on regional consequences of our results because we are aware that our results are limited to the specific region and may not be easily generalized. We will add more information regarding catchments (e.g. by including a table with catchment characteristics as mentioned in one of your comments above) and we will consider reorganization of the discussion section to better highlight regional differences between our study catchments.

Conclusions: I'd appreciate if you could relate the statements with the according figures, that makes it easier for the reader to recap on where to find the evidence for the conclusions

Thank you for the suggestion. We will add some relevant links to results and figures.

The second objective (lines 86 & 87) is to show the importance of snowmelt "at different elevations", however elevation differences where not really mentioned and I also did not find any concluding remarks regarding this statement.

As we mentioned in one of the comments above, the dependence of individual characteristics on elevation is rather indirect and may not be easy to interpret although the elevation clearly influences the snowfall fraction and thus snow storages. We agree that mentioning the elevation as the most important catchment attribute might be confusing. We will reformulate both objectives and discussion to be clearer.

I am also not convinced that I saw results that support that "future liquid precipitation will not compensate the lower solid precipitation", please re-write or leave out.

With this sentence, we wanted to draw the attention on the fact that future decrease in snow storages might cause a decrease in annual runoff (even despite no changes in total amount of precipitation) and we think it is important to mention it. But maybe the formulation is not fully clear, so we will reformulate it to be clearer.

References

Carroll, R. W. H., Deems, J. S., Niswonger, R., Schumer, R. and Williams, K. H.: The Importance of Interflow to Groundwater Recharge in a Snowmelt-Dominated Headwater Basin, Geophys. Res. Lett., 2019GL082447, doi:10.1029/2019GL082447, 2019.

Staudinger, M., Stoelzle, M., Seeger, S., Seibert, J., Weiler, M. and Stahl, K.: Catchment water storage variation with elevation, Hydrol. Process., 31(11), 2000–2015, doi:10.1002/hyp.11158, 2017.