Review
Title: What drives Fyw variations with elevation in Alpine catchments?
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General comments

Gallart et al. address the scientific questions of "… *what drives Fyw variations with elevation in Alpine catchments clarifying why Fyw is low at high altitudes*» (L20). For this, the authors combine existing and new F_{yw} values from Switzerland and Italy and compare them with several other variables that describe snow cover, baseflow conditions, and geology. From these comparisons the authors develop a perceptual model, suggesting that a longer persistence of the seasonal snowpack results in deeper groundwater flow paths and thus smaller F_{yw} values, in contrast to hybrid catchments with ephemeral snow packs. The authors also present a new classification scheme to identify a catchment's hydro-climatic regime. The analysis of the used data is thorough and most figures are clear and informative. The analysis of satellite images to explore the linkages between snow cover duration and F_{yw} are certainly interesting. However, I would like to encourage the authors to highlight more the novelty of their findings and the scientific contribution of their work, considering that they cite several papers in which comparable analyses have been carried out and similar conclusions (with respect to flow and storage processes) have been reached.

I think that the **research objectives** (or research questions) should be formulated more explicitly in the Introduction in order to guide the following analysis. It is not clear whether the authors attempt to explain the scatter in the F_{yw}-gradient relationship (L76), the low F_{yw} values in steep and/or high-elevation catchments (L79), or both.

L156 "*we classify the catchments in the three hydro-climatic regimes (snow-dominated, hybrid and rainfall-dominated) proposed by Staudinger et al. (2017), but we introduce a new formal criterion of classification*": Why is a **new definition of the catchments' hydro-climatic regimes** needed? As far as I can tell, only two catchments, BIB and GUE, were newly classified. The new sites outside of Switzerland could have easily been categorized as hybrid or snow-dominated based on their streamflow and topographical data. Furthermore, the discussion of this new classification scheme (Sect. 4.2 and 5.1) somewhat distracts from the main topic of the paper, which is the investigation of small F_{yw} in high-elevation catchments.

I was surprised to see that the authors did not include annual or seasonal **precipitation** in their analysis. This variable should be tightly related to F_{bf} and F_{SCA}. Annual precipitation is also very low at some Swiss high-elevation sites, which would also explain why F_{yw} is low there. What is the reason for not considering precipitation at all?

The important aspect of **snow pack storage** in high-elevation, snow-dominated catchments, which the authors only touch on in the Conclusions section, should instead be brought up much earlier in the manuscript. In fact, it has been discussed already in another paper: «*Another analytical decision that affects the interpretation of Fyw* and Fyw relates to whether snowpack storage is considered to be part of catchment storage, or not. If one measures precipitation to the snow surface as the catchment input, then snowpack accumulation and melt are implicitly included in catchment storage (e.g. Staudinger et al., 2017). In this case, comparisons of seasonal cycles in precipitation and streamflow should reflect the young water fraction resulting from the combination of snowpack and subsurface storage. Alternatively, if one uses precipitation and snowmelt arriving at the soil surface as the catchment input (for example, with melt pan lysimeters, or modelled snowpack out- flows), then snowpack accumulation and melt are implicitly excluded from catchment storage. In this case, comparisons of seasonal cycles in streamflow and sub-snowpack catchment input should reflect the young water fraction resulting from subsurface storage alone. Because the total catchment storage in the first case (including snowpack storage) is larger than the subsurface storage alone, <u>the resulting young water fractions are expected to be smaller</u>.*» (von Freyberg et al., 2018). In addition, in high-elevation catchments with perennial snow packs, snowmelt in spring and summer is likely to be older than 2-3 months (because the snow fell more than 3 months before the melt occurs). As a result, although summer discharge might be high it will consist mainly of old snowmelt and groundwater rather than recent rainfall (i.e., F_{yw} is small). In hybrid and rain-dominated catchments, streamflow receives relatively more young water from young snow packs and recent rainfall events, respectively.

The authors seem to overlook this storage aspect of the snowpack and instead focus mainly on the groundwater contribution to streamflow (L82). A main finding of the paper is a strong negative correlation between the baseflow fraction F_{bf} and F_{yw} (Sect. 4.3.3, Fig. 10) from which the authors derive several statements which I'd like to comment on (Sect. 5.4):

L553: "*We find the highest F_{bf} for snow-dominated catchments confirming the presence of high subsurface storage, contributing to streams, in high-elevation catchments*». I would include the snowpack as part of the storage here because winter precipitation is stored in the snowpack until summer when it recharges aquifers or runs off into the stream.

L554.: "*Moreover, the annual baseflow is strongly positively correlated with the F_{SCA} (ρSpearman= 0.81 p-value < 0.01) suggesting a major groundwater contribution with increasing snow cover persistency (Fig. S6)*». This depends strongly on your baseflow estimation method. Further, increasing baseflow and snow cover persistency are both results of increasing catchment elevation and/or annual precipitation. Thus, baseflow cannot simply be linked to snow cover persistency.

L558: "*The hydro-climatic regime is generally a good indicator of the proportion of young water that contributes to streamflow...*" What does this mean exactly? If the authors refer to Tab. 3, there is quite some overlap between the rainfall- and snow-dominated regimes with respect to F_{yw}, and thus F_{yw} cannot be estimated from the regime types alone.

L570 (&L37): "*Therefore, we can conclude that the contribution of groundwater storage to streamflow, which is driven by snowpack duration, can be considered as the best explanatory variable of the F_{yw} elevation gradients*." Again, I would rather argue that not snowpack duration but rather storage capacity (both in the subsurface and the snowpack) together with the hydro-climatic conditions (P-ET) and catchment properties affect the contribution of old water (not necessarily only groundwater) to streamflow, and thus F_{yw}. In high-elevation catchments, the snowpack can function like a subsurface water storage that releases (>3 months) old water during the melting season. This old water is meltwater, not groundwater and I suspect that the baseflow separation method used in this paper is not able to differentiate between the two.

Based on the analysis of **slope data** the authors conclude that (L370) *"... that there is an increasing rate of infiltration when the hydro-climatic regime transitions from hybrid to snow-dominated*.". I don't think that this statement is well supported by using slope data in Fig. 4 (no data on infiltration is provided). Instead, the only conclusion that can be drawn from the data presented in this manuscript is that the hybrid catchments receive more precipitation than the rain-dominated catchments (L478), resulting in more recent precipitation becoming streamflow, i.e. higher F_{yw} values. This is analogous to earlier findings in von Freyberg et al. (2018): *"... young water fractions tend to be highest in humid catchments where prompt runoff response is facilitated by fast flow paths and/or high-intensity precipitation events*."

One outcome is a "*perceptual model of how snow persistency explains F_{YW} during winter and summer along topographic gradients*". This **model**, presented in Fig. 13, tries to summarize the combined effects of catchment properties (steepness, elevation) with processes (ET, wetness, snowmelt). The resulting figure is very complex and difficult to understand. For instance, if a reader seeks to understand the figure without reading the entire paper, is not clear as to what "*increases/decreases with elevation*" means. Does this refer to increases/decreases of F_{yw} within a single catchment or between different (high- to low-elevation) catchments?

Specific comments

The **title** of the manuscript does not well reflect the content of the paper. It rather gives the impression that F_{yw} was studied along elevation gradients within (individual) catchments. In addition, the term "Alpine" suggests that solely mountainous catchments within the Alps mountain range were considered, however, catchments such as ERG, AAB and MEN are located in the Jura Mountains and Swiss Plateau, respectively. It would be nice to define early on what is meant here by Alpine, given that the Introduction starts with the general statement (L41) "*Alpine catchments are assumed to generate a high share of surface runoff* ..."

Ideally, the **time periods** that were used to calculate the various metrics should be the same as those of the isotope data used to calculate F_{yw}. As far as I can tell, this has been considered only for F_{bf}, whereas F_{SCA} was determined based on satellite data from 2017-2021. For WFI and Q_{June}/Q_{DJF}, no information is provided. The F_{yw} values in von Freyberg et al. (2018) only cover the time periods 2010-2015, which is not even overlapping with the satellite images used to determine F_{SCA}. I would like to encourage the authors to compare data only from the same time periods, especially when these periods included extremely dry/wet climatic conditions.

The terms **elevation and steepness** should not be used synonymously, as in L361:" *Initial evidence of low Fyw in high-elevation catchments is given in the work of Jasechko et al. (2016). Based on the analysis of 254 worldwide watersheds, their work reveals a reduction of Fyw in steeper terrains.*" Also, low-elevation (rainfall dominated) catchments can be very steep, and there surely exist high-elevation (snow-dominated) catchments with flat topography.

When I look closer at the **f_{SCA} time series** (Fig. 5), I wonder how it is possible that the AAC catchment at around 500m asl. was almost entirely snow covered in summers of 2018 and 2020 (f_{SCA} around 1)? The same is true for the catchments BIB and ERL where the snow cover usually disappears by June each year. Can it be that f_{SCA} tends to be over-estimated with your approach? I would also expect F_{SCA} to be strongly correlated with (mean) catchment elevation so that elevation instead of F_{SCA} could be used in your analysis. As can be seen in Fig. 12, a similar grouping of catchments emerges.

L275 mentions that "*The Noce Bianco Pian Venezia (NBPV) catchment is an exception since it generally has snow over the glacier also during summer.*». As far as I remember, the catchments VdN, DIS and OVA are also partially glacierized. Should they be considered as exceptions as well?

Fig.10: A very similar result is presented already in von Freyberg et al. (2018) where F_{yw} and the quick-flow index QFI, the inverse of the baseflow index, showed a significant positive correlation (note that the QFI and $1/F_{bf}$ will likely not be exactly the same, although both were calculated through digital filtering of discharge time series).

L484: "*In addition, higher order channels, higher up, are more rarely activated than lower order channels that are more often active*" If the authors refer to Strahler stream orders here, higher elevation streams usually have low Strahler orders (starting with first-order streams). The Strahler stream orders increase downstream.

L560-565: Why was BCC not classified as snow-dominated, based on the evidence from previous research?

L566-569: Is it possible that precipitation isotopes in the NBPV catchment were sampled differently compared to the other catchments in this study, e.g. with a heated precipitation collector? This could result in a larger A_S value. Can the authors confirm that the precipitation isotope sampling in the snow-dominated catchments was comparable across all sites?

L596: "*…leads to high baseflow throughout the year...*». This contradicts the data shown in Fig. 9. I would suggest to replace 'baseflow' with 'baseflow fractions F_{bf}'.

Technical comments

The language of the manuscript is often not precise and needs to be improved. Some sentences are difficult to understand, e.g.

- (L310) "*Additionally, Duncan (2019) provides a specific technique that allows estimation of separate components with physical relevance in the case that baseflow separation techniques were not applied to describe physical processes*." This sentence is redundant and not scientifically specific (e.g., what are "separate components with physical relevance"?).

- (L33) "*Finally, our work highlights that Fbf, considered as a proxy for groundwater flow, is roughly the one's complement of* F_{yw}". Isn't F_{bf} rather a proxy for the groundwater contribution to streamflow? It does not provide any information about flow processes. What does "*roughly the one's complement of* F_{yw}« mean?

- L34 «*...we find high Fbf during all low-flow periods, which underlines that streamflow is mainly sustained by groundwater in such flow conditions*.» That high F_{bf} represents a major contribution of groundwater to streamflow is implicit in the method of Duncan (2019). This is not a new finding.

- L496 "*the temporal dynamic of snow accumulation and melt and its effect on deep infiltration supports the pivotal role of snowmelt in recharging groundwater during summer in high-elevation environments ...*" This sentence is redundant. *Snow melt affects deep infiltration* is equivalent to *it plays a role in recharge*.

Sect. 3.2: To indicate whether a variable was flow weighted, earlier papers have added a "*", and thus I would suggest to write F_{yw}^* and A_S^* here as well.

L328: Please verify whether the flow-weighted young water fraction of SOU is indeed 0.01. If so, the following statement "*while flow-weighted Fyw remains unchanged for the very small lateral subcatchment*" is false.

References

Duncan, H. P.: Baseflow separation – A practical approach, J. Hydrol., 575, 308–313, https://doi.org/10.1016/j.jhydrol.2019.05.040, 2019.

von Freyberg, J., Allen, S. T., Seeger, S., Weiler, M., and Kirchner, J. W.: Sensitivity of young water fractions to hydro-climatic forcing and landscape properties across 22 Swiss catchments, Hydrol. Earth Syst. Sci., 22, 3841–3861, https://doi.org/10.5194/hess-22-3841-2018, 2018.