Dear Jana von Freyberg,

thank you for your care and attention during your reading of the manuscript, your positive remarks and your comments that will help to improve the work. Please find below the responses to all your comments.

We will take into account all your constructive feedback in the revised version of the manuscript once we receive the editor's response.

With kind regards,

The Authors

General comments

Gentile et al. address the scientific questions of "… what drives F_{yw} *variations with elevation in Alpine catchments clarifying why* F_{yw} *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.*

Thanks for the positive overall assessment.

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.*

We will make the research objectives clearer in the introduction. The research question that motivated our work is: What are the hydrological processes hidden behind the low young water fractions in high elevation catchments? Our main hypothesis is that this relationship can at least partially be explained by snow cover persistence and quaternary deposits. These factors were not previously considered in the scientific literature for explaining variations in F^*_{yw} **('*' indicates a flow-weighted variable) at different elevations:**

- **The snowpack persistence (quantified with** F_{SCA} **in our work) is hereby seen as an essential factor that drives the duration of the low-flow period at high elevation sites, i.e., of the period where streamflow is only fed by groundwater (water that in terms of age is old water). In this context, we decided to also consider, as an explanatory variable, the groundwater contribution to streamflow. In this regard, we have applied a recent baseflow filter that emphasizes the physical relevance of the separated flow components (Duncan, 2019) (i.e., it describes the physics of baseflow better than other filters).**

- **Quaternary deposits are thought to be a factor that influences potential subsurface storage that contributes to the stream (Arnoux et al. 2021).**

The potentially dominant water flow and storage processes driving young water fractions are of course discussed in previous work (e.g., Jasechko et al. 2016, von Freyberg et al. 2018, Lutz et al. 2018); to the best of our knowledge, this is the first attempt to present a perceptual model that harmonizes these known processes with the surprising low F^*_{yw} values of high elevation catchments. This will be explicitly stated in the introduction.

In the analysis, we have also considered catchments at low elevations (rain-dominated) for having a complete vision about the dominant hydrological processes at different elevations, but the results presented in the pre-print do not convey new insights into the knowledge of F^*_{yw} variations at low-elevation sites. We have in particular not explored the role of the low-flow period length (which cannot be related to the snow cover persistence); but could have a key role in driving F^*_{yw} also at lower elevation sites. We will deepen this point in the revised version of the manuscript.

Below, we give some more detailed justification for our hypothesis about the link of low flow and F^*_{yw}. This will also be made clear in the paper:

F^*_{yw} predicts the flow-weighted average over a certain time-interval (Kirchner 2016a, 2016b):

$$F*_{yw} \simeq \frac{\sum_{i=1}^{n} Q(t_i) F_{yw}(t_i)}{\sum_{i=1}^{n} Q(t_i)} \qquad \textit{Eq. (1)}$$

where n is the number of days in the considered time-interval, Q is the daily discharge, $F_{yw}(t_i)$ is the daily young water fraction. As is clear from this equation, F^*_{yw} becomes low if either $F_{yw}(t_i)$ is low for a high flow or if $F_{yw}(t_i)$ is very low for many time steps or both.

Low flow situations happen when the streamflow is baseflow-dominated (i.e. groundwater dominated, i.e. old water dominated), i.e. we can anticipate that low $F_{yw}(t_i)$ occur together with low $Q(t_i)$ and that $F_{yw}(t_i)$ is higher during high flow periods. The overall effect upon F^*_{yw} remains thus a priori unclear. It is however tempting to think that i) the duration of low flow periods or ii) the share of baseflow could explain F^*_{yw}. Indeed, a plot of F^*_{yw} against the duration of low flow periods (where a low flow period is defined as a period when 85% of the total flow is composed of baseflow) shows a strongly negative correlation (Fig. 1a below). Moreover, a plot of low-flow duration against elevation shows a decrease until 1500 m asl and an increase thereafter (Fig. 1b) (i.e., an opposite behavior with respect to F^*_{yw} against elevation, see Fig. 12b of the pre-print). The NBPV does not fit the overall picture (Fig. 1b)

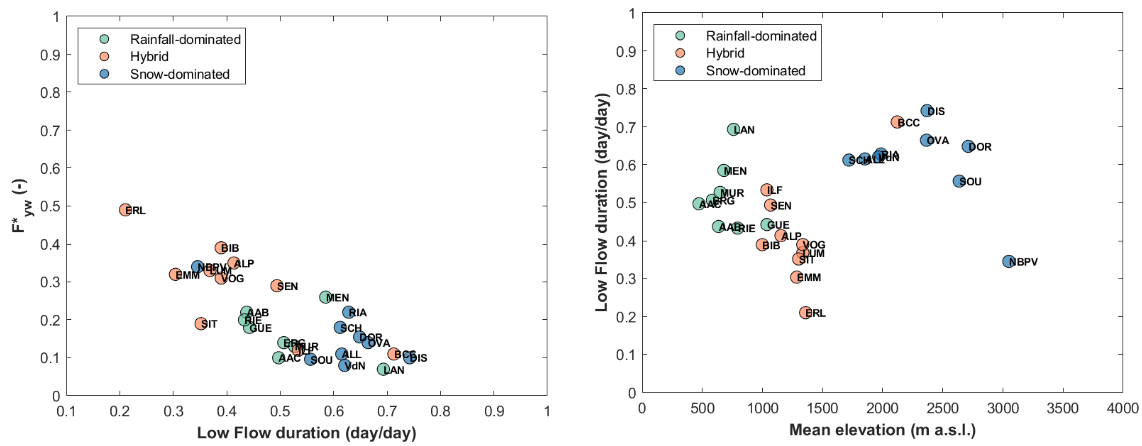

Figure 1. a) young water fraction against low-flow duration b) low-flow duration against mean catchment elevation

Baseflow filters were applied in previous studies and results were correlated with F^*_{yw}. However, applying the Duncan (2019) baseflow filter (with the parameter suggested by Nathan and McMahon, 1990, without any type of calibration) to the discharge data of our catchments, the estimated F_{bf} is roughly the complementary term of F^*_{yw}:

$$F^*_{yw} + F_{bf} \simeq 1 \qquad \text{Eq. (2)}$$

We better show this complementarity in Fig.2a and Fig.2b (below) and will include relevant aspects in the revised version. In these figures, we added the F_{bf} uncertainty calculated through a Gaussian error propagation, considering the baseflow filter parameter as distributed according to a Gaussian distribution.

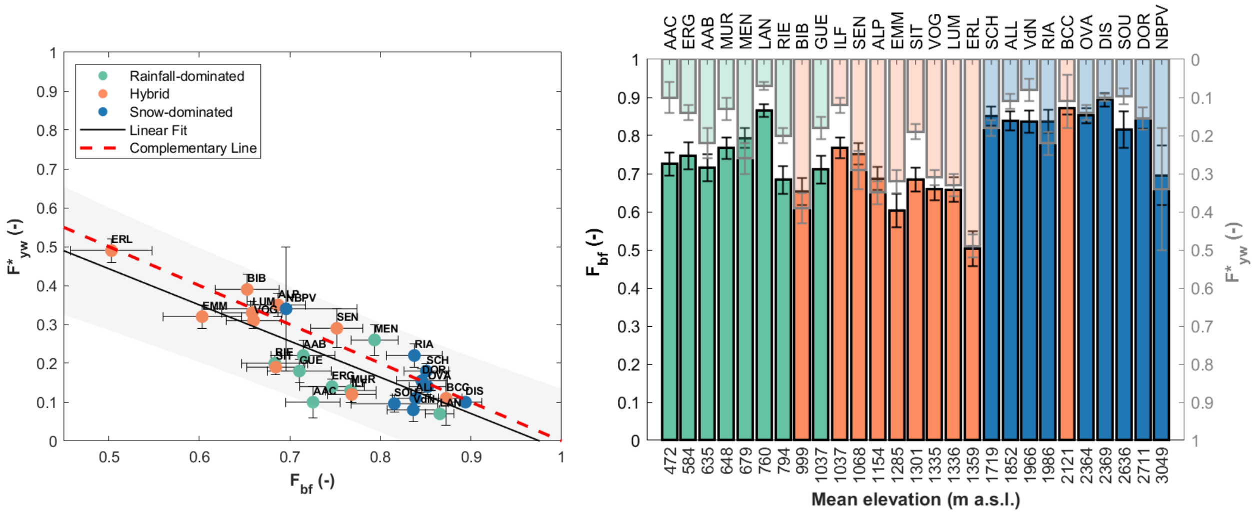

Figure 2. a) young water fraction against fraction of baseflow b) complementarity between F_{bf} and F^*_{yw}.

This result indicates that F^*_{yw} could potentially be estimated as: $F^*_{yw} \simeq 1 - F_{bf}$, without the application of the amplitude ratio approach using stable water isotopes. This could be a useful result for catchments in which stable water isotopes measurements are not available. Of course, future studies could compare the F_{bf} with F^*_{yw} for new catchments to validate or refute this result in different hydroclimatic conditions or geologies.

In summary: we will make the research questions and hypotheses clearer throughout the paper and emphasize on the novelties in the conclusion and the abstract.

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.

Thank you for this comment, which was already made by reviewer 1. We copy here the answer that we gave to reviewer 1:

"We propose a new criterion for the regime classification because our dataset includes catchments outside the Swiss borders (i.e., the four Italian catchments) for which the Weingartner and Aschwanden (1992) and Staudinger et al. (2017) classification scheme cannot be strictly applied since they were designed for the Swiss hydro-climatic regimes. We "manually calibrate" the thresholds of F_{SCA} and Q_{June}/Q_{DJF} for classifying catchments in "rainfall-dominated", "hybrid" and "snow-dominated" as in the work of Staudinger et al. (2017). In this way, the classification scheme is "calibrated" on the Staudinger et al. (2017) catchments and we can apply it also outside the Swiss borders. According to the referees' comments, we will consider the possibility of modifying the classification scheme to make it more straightforward to link to previous classification (e.g., using streamflow and topographical data), but it will remain transferable to other regions."

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?

We decided to use variables that were not previously considered for explaining young water fractions variations; Jasechko et al. (2016) wrote: "Although topographic gradient provides the strongest correlation with young streamflow fractions in our data set, the fraction of unexplained variance is large, suggesting that other variables also play a significant role. *<u>We observe no significant correlations between the young streamflow fraction and</u>* catchment size, *<u>annual precipitation</u>*, bedrock porosity, population density, or the fraction of catchment area comprised of pasture land or open water".
We explain in the manuscript that, below roughly 1500 m, the increase of F^*_{yw} with elevation also depends on the increase of precipitation with elevation. In fact, in such cases, annual precipitation can be considered as a proxy of catchment wetness since we mainly observe a liquid water input.
Above 1500 m, using mean annual precipitation as a proxy for catchment wetness is misleading because the seasonal snowpack leads to a very dry period of the year despite high (solid) water input. In other words, the total amount of precipitation is not the variable of interest, rather the temporal concentration of water input is the relevant variable. It is possible to observe the saturation of the system (i.e., high wetness conditions) also when annual precipitation is low if a large volume of water (stored in the snowpack) is released in a relatively concentrated time interval. After the long winter period, we expect high infiltration that recharges the groundwater storage. This process can bring the system to saturation (high wetness) so that ultimately rain or snowmelt can more rapidly reach the stream as overland flow.
Additionally, Lutz et al. (2018), estimating the young water fraction for 24 catchments in Germany, have found exactly the opposite of what is generally thought: the young water fraction decreases with increasing mean annual precipitation. They stated that this result reflects "the impact of various factors relevant in the mountainous region, resulting in the decrease of young water fractions for higher-elevation catchments" (Lutz et al. 2018). Thus, what we did in our work is searching for the "various factors" that lead to low F^*_{yw} in mountainous catchments not considering the mean annual precipitation as an explanatory variable.

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 F^*_{yw} and F_{yw} 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, the resulting young water fractions are expected to be smaller.» (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.*

Thank you for this comment. As reported in von Freyberg et al. (2018), the young water fractions are virtually identical between "direct" and "delayed" input, but, of course, there is a "conceptual" difference in using the "direct" or "delayed" input, which we omitted to discuss in the pre-print. We will take this in account for the revised version of the manuscript.

Considering our hypothesis (supported by scientific literature) that snowmelt mainly transits through the groundwater store, we will estimate, in the revised version, the F^*_{yw} with the "direct" input (as suggested in the comment to L553): i.e., we will consider the snowpack as part of the catchment storage. Moreover, we will bring up the role of snowpack storage earlier in the text and we will cite that the role of snowpack storage in high-elevation, snow-dominated catchments has been discussed already in the work of von Freyberg et al. (2018), in which the authors state that, including the snowpack storage in the catchment storage, the resulting young water fractions are expected to be smaller. However, we will clarify in the revised version that the main aim of this work is not to focus on how the snowpack affects the F^*_{yw} estimation in a single catchment, since it was treated by previous works (e.g., von Freyberg et al. 2018, Ceperley et al. 2020), but to describe what are the hydrological processes (also related to the snowpack storage) hidden behind the F^*_{yw} variations with elevation gradient, focusing on low F^*_{yw} at high elevations.

We agree that, for snow-dominated systems, 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). We also agree that summer discharge will consist mainly of <u>old snowmelt (or groundwater)</u> rather than recent rainfall (i.e., F^*_{yw} is small).**

****However, we believe it is not correct to distinguish between old snowmelt and groundwater. We know that recent snowmelt is likely to be older than 2-3 months and we also know that, according to several papers, recent snowmelt has a key role in recharging the groundwater storage during summer. Therefore, groundwater storage is assumed to be mainly composed of old snowmelt. If the groundwater storage contributes to the stream (mainly during the long winter low-flow period, but also during summer), this contribution will reduce the F^*_{yw}.**

The authors seem to overlook this storage aspect of the snowpack and instead focus mainly on the groundwater contribution to streamflow (L82).

Thanks for this important comment. In addition to our above answers, it is of prime importance to point out here and in the paper that large parts of the snowmelt actually transit through the groundwater storage: i) the very high baseflow in high mountain catchments during summer is a direct sign of this fact. ii) groundwater in such catchments often has the isotopic signature of snowmelt (Michelon et al., HESSD paper, others). We will make this much clearer in the paper. Please see also our answer to the community comment by Jansson, the general comment number (1) on the relation between F^*_{yw} and F_{bf}.

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.

Thank you for this comment. You are right that we have not pointed out the fact that, in our analysis, we are assuming that the snowpack is part of the catchment storage. We will make this coherent with the estimation of F^*_{yw} through the "direct" input. We will include it and mention the relation between snowmelt and groundwater.

L554.: "Moreover, the annual baseflow is strongly positively correlated with the F_{SCA} ($\rho_{Spearman}$= 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.

Calculating the annual baseflow through another baseflow filter (e.g., the Lyne and Hollick, 1979) the positive correlation with F_{SCA} does not change (Fig. 3a and Fig. 3b).

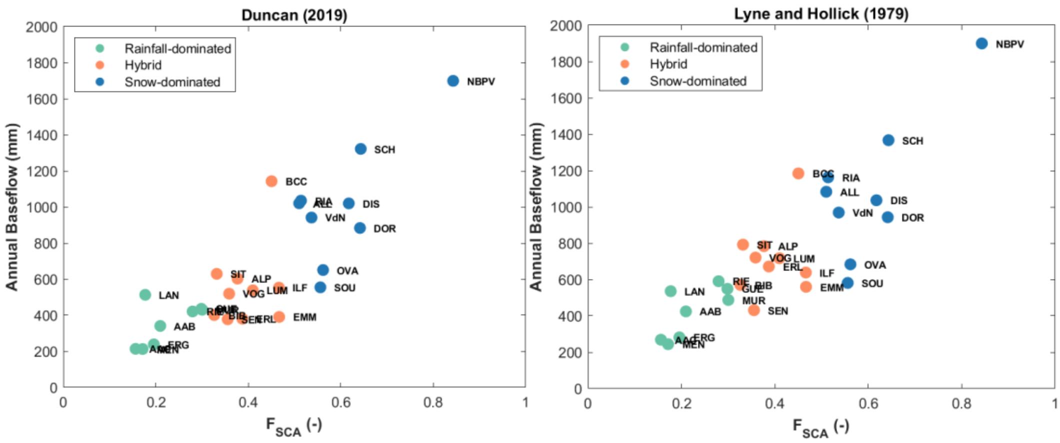

Figure 3. Annual baseflow against F_{SCA} considering two different methods of baseflow estimation: a) Duncan (2019), b) Lyne and Hollick (1979).

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.

We are not sure if there is literature that shows that baseflow increases as a function of catchment elevation but that's indeed what we find in our work (see Figure 2b). In general, authors assume that low elevation catchments have more groundwater storage potential because of large alluvial aquifers.

Baseflow could increase with precipitation (given that there is enough storage potential in the subsurface), but in Switzerland, a general increase of precipitation with elevation is observable only up to 1600 m asl., at higher elevation the trend is unclear (Sevruk, 1997).

Finally, we would like to underline that we do not pretend that snow cover persistence alone explains baseflow, we simply show the statistical link. This will be made clear.

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.

We are saying that through the regime type one could roughly say the "order of magnitude" of F^*_{yw}. However, since this sentence can create misunderstanding, we will reformulate or remove it.

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.

We will make the link between snow cover, groundwater and baseflow much clearer in the revised version. We agree that snow releases old water, but groundwater is largely composed of snowmelt in these systems. The sustained high flows in July in high elevation catchments (without glacier) are not the result of continuous overland flow composed by meltwater, but the result of groundwater release, i.e. of groundwater that was previously recharged by melt. We thus cannot distinguish between snowmelt and groundwater. But with the help of the baseflow ratio, F_{bf}, we can quantify the share of streamflow that is due to groundwater release; the share of snowmelt (with age > 3 months) that flows off quickly will not show up in F_{bf} and, in many sites, this could explain the "residuals" of 1- (F_{bf} + F^*_{yw}). We quote here for completeness also our answer to general comment [1] of Jansson:

"We agree that in snow-free systems, F^*_{yw} is by definition related to F_{bf}, the ratio of baseflow in annual flow: baseflow is composed of groundwater and groundwater is the dominant source of old water in snow-free systems (in absence of large lakes). However, in snow-influenced systems, part of the old water is temporarily stored in the snowpack. This is at a first glance not measured by F_{bf}. We nevertheless show here that F_{bf} obtained from streamflow alone (with the selected baseflow filter) leads to an approximately complementary relationship to F^*_{yw} (F^*_{yw} + $F_{bf} \simeq 1$), which is an important result for catchments where we do not have isotope measurements. Why is this so? Simply because the snowmelt in snow-dominated systems largely transits through

the groundwater store and leads to high baseflow. This will be made much clearer in the revised version."

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."

Thank you for this comment. Yes, we do not have infiltration data and probably this conclusion only using slope data cannot be well supported, also if Jasechko et al. (2016) concluded that in steeper terrain the low F^*_{yw} could be caused by rapid percolation through fractures and deep flow paths (as also reported in Lutz et al. 2018). We will change this part in the manuscript incorporating the conclusion you suggest and citing the relative paper.

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?

We realize that Fig. 13 can be misleading since only a single catchment is represented. Therefore, we will work to improve this figure to better reveal our "step forward" regarding the hydrological processes behind the F^*_{yw} variations between different 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 …"

Thank you for this comment. We will work on the title to avoid reference to Alpine when we talk about all studied catchments.

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.*

We agree. In fact, F_{bf} and *WFI* were calculated in the same time period of isotope sampling, this will be specified. F_{SCA} is calculated in the period 2017-2021 simply because of the availability of the Sentinel-2 satellite images (there are no Sentinel-2 images in the period 2010-2015). We will make this clear.
For the Q_{June}/Q_{DJF}, we used a long-term average since this ratio was used for a classification purpose. We will make the retained time periods explicit in the revised version.

The terms elevation and steepness should not be used synonymously, as in L361:" Initial evidence of low F_{yw} 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 F_{yw} 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.

We agree. We will carefully review the language.

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?

Our estimation algorithm indeed suffers from overestimation (see our answers to reviewer 1) and will be modified in the revised version.

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.

A priori, we could imagine a strong correlation between these two variables, but elevation is not an explicative "tool" for the processes we focus on in the manuscript. In fact, it could be approximative to describe the snow cover persistence only with the increasing elevation: the persistence of snow in a catchment also depends on catchment's aspect, topography, (Painter et al. 2023), snow-related and climatic characteristics. In fact, catchments with very different characteristics (e.g., different elevation ranges, different areas etc.) can reveal a similar mean elevation, but the snow persistence could considerably change. Thus, we need a "tool" that is directly linked to the processes we want to describe and the F_{SCA} is a variable that, of course, is related to the mean catchment elevation, but it is "better physically related" to the snow-cover persistence.

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?

We consider only NBPV as an exception because 42% of its area is covered by glaciers. DIS only 2 %, VdN 3 %. For OVA we see it is not covered by glaciers (van Tiel et al., 2019). Thus, for the other catchments, we consider negligible the effect of glaciers on F^*_{yw}. We will make clearer the high glacier-cover ratio of NBPV.

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

Thank you for this, we will cite, in the discussion about Fig. 10, that a similar result was found by von Freyberg et al. (2018). However, we want to underline that Duncan (2019) improved the baseflow filter of Lyne, V. D. and M. Hollick (1979) [BaseflowSeparation, EcoHydrology package in R], used by von Freyberg et al. (2018), to separate flow components with physical relevance (Duncan, 2019). von Freyberg et al. (2018) found a positive correlation between F^*_{yw} and *QFI:* average of $(Q-Q_{bf})/Q$ (where Q_{bf} is obtained with Lyne, V. D. and M. Hollick, 1979 baseflow filter). However, F_{bf}, average of Q_{bf}/Q = average of *1-QFI*, is not complementary to F^*_{yw} (i.e., F^*_{yw} + average of $(1\text{-}QFI) \neq 1$). A good result of our work is that, using the baseflow filter parameter commonly proposed in literature (Nathan and McMahon, 1990), the Duncan (2019) baseflow filter returns a F_{bf} that is roughly complementary to F^*_{yw} (i.e., $F^*_{yw} + F_{bf} \simeq 1$) without any type of calibration. This could be a very useful result for catchments in which isotopes measurements are not available. In these cases, F^*_{yw} could be estimated as $1\text{-}F_{bf}$.

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.

Thanks for pointing out our language mistakes.

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

This was because of the new classification scheme we propose in the paper. We will come up with a final conclusion about the classification scheme in the revised version.

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?

Thank you for suggesting the clarification of the approach used for sampling precipitation. In NBPV catchment, we did not use a heated precipitation collector. Bulk samples of rain water were collected monthly at the outlet of the catchment by 5-L bottles equipped with a funnel and a layer of mineral oil to prevent evaporation, whereas snow samples were collected using an aluminum cylinder, inserted vertically from the surface to a depth of 20 cm (Zuecco et al., 2019). We applied the same sampling approach of precipitation in NBPV and BCC (Penna et al., 2016). For VdN "bulk rain samples were collected for isotopic analyses using funnels flowing into insulated bags at three locations corresponding to the rain gauges (1,253, 1,500 and 2,100 m a.s.l.), and emptied weekly or biweekly between June 2016 and November 2018. Between February 2016 and April 2018, snow samples were collected from the entire snow profile at various locations in the catchment" (Ceperley et al. 2020). For DOR and SOU precipitation samples were collected at a monthly resolution using a double rain and snowfall isotope sampler installed on a pole 3.7 m high. Therefore, we consider the precipitation isotope sampling comparable across all the new sites, while precipitation isotopes in the 22 sites of von Freyberg et al. (2018) are modeled through an interpolation method.

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}'.

Thank you for this suggestion.

Technical comments

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

Thank you for all technical comments and for having taken the time to report the language issues. We will improve and clarify the language over the entire manuscript.

- *(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"?).*

The separate components with physical relevance are baseflow and quickflow (i.e., total flow minus baseflow).

- *(L33) "Finally, our work highlights that F_{bf}, 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?*

Thank you for this comment. We consider F_{bf} as a proxy for the groundwater contribution to streamflow knowing that the used baseflow separation method is able to describe how much is relevant the baseflow (or groundwater flow) on the entire hydrograph.

The meaning of "roughly the one's complement of F_{yw}" is that, for each catchment, $F_{bf} + F^*_{yw} \simeq 1$.

- *L34 «...we find high F_{bf} 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.*

Thank you for this comment. We simply wanted to underline what was the meaning of high F_{bf} during low-flow periods for a general reader. However, we can omit the second part of this sentence.

- *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.*

Thank you for this. We will modify this sentence as: "the temporal dynamic of snow accumulation and melt supports the pivotal role of snowmelt in recharging groundwater during summer in high-elevation environments ..."

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.*

Thank you for this suggestion. We will use this notation in the revised manuscript.

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

Thank you for noticing this. It is simply an error in the text: the flow-weighted young water fraction of SOU is 0.1, not 0.01 and the statement "while flow-weighted F_{yw} remains unchanged for the very small lateral subcatchment" is true.