

Author's response: a list of all relevant changes made in the manuscript and a point-by-point response to the reviews.

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Dear Editor and Referees,

we would like to thank you for both the overall appreciation of our work and the appreciation of our plan to revise it.

20 Considering the referees' comments, the Editor decided that major revisions are necessary before the review process can be continued. The referees' comments have been very constructive for the paper improvement and have been the main drivers of our changes. Accordingly, you will find major changes in the revised manuscript. We have addressed all the issues raised in the interactive discussion, including the language improvement. To meet all the referees' requests, many sections have been deeply reorganized and rewritten, as it is possible to see in the track-changes version of the manuscript.

25 The present document is subdivided in two Sections. In the first section we summarize all the major changes applied to the submitted document you have revised. In the second section we report a point-by-point response to the reviews.

In the hope of having met your scientific expectations in the revised manuscript, we kindly ask you to reconsider the publication of our work on the Hydrology and Earth System Sciences Journal.

30 With king regards,

The Authors

1 List of all relevant changes made in the manuscript.

35 1.1 Title: relevant changes

Jana von Freyberg commented: “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...”.

40 Starting from this comment we have decided to change the paper title:

From: “*What drives F_{yw} variations with elevation in Alpine catchments?*”

To: “*Towards a conceptualization of the hydrological processes behind changes of young water fraction with elevation: a focus on mountainous alpine catchments.*”.

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We think that this title reflects much better the content of the paper, which is concluded with the presentation of a perceptual model, the first step before conceptualization (Beven, 2012), that integrates all the results of our analysis to describe a framework for how hydrological processes control the F_{yw}^* according to elevation. In the title, we underline that the focus of the paper is on mountainous alpine catchments. We use the term “alpine” instead of “Alpine” to refer to the typical hydro-
50 climatic conditions of a mountain climate. In other words, we are not specifically referring to the Alps Mountain range. Finally, we have changed “ F_{yw} ” to “young water fraction” in the title as suggested by the referee #1: “... “ F_{yw} ” could be changed to “young water fraction” for general readers”.

1.2 Abstract: relevant changes

We highly reformulate the Abstract to better communicate the scientific contribution of our research paper and the novelty of
55 our findings, as suggested by Jana von Freyberg: “I would like to encourage the authors to highlight more the novelty of their findings and the scientific contribution of their work”.

1.2.1 Scientific contribution

- We express the research gap about the missing of a harmonious framework of hydrological processes explaining low young water fraction in mountainous catchments and, accordingly, that our aim is to give an overview of what
60 drives the young water fraction variations according to elevation, thus clarifying why it generally decreases at high elevation.

1.2.2 Novelty

- We communicate in the Abstract that we provide the above-mentioned missing framework through our perceptual model.
- 65 - Our novel findings reveal that the low-flow duration (LFD), quantified in the revised version of the manuscript, is the main driver of F_{yw}^* variations with elevation and that it is very high in high-elevation catchments because of the persistence of the winter seasonal snowpack which promotes the groundwater (or old water) storage emptying which sustains the streams during winter.
- 70 - We highly stress in the Abstract the usefulness of the complementarity between F_{yw}^* and F_{bf} (we have found in this work) to potentially estimate F_{yw}^* in catchments where stable water isotopes measurements are not available.

1.3 Introduction: relevant changes

We have applied major changes to the Introduction. The main info presented in the submitted version remain in the revised Introduction, but the integration of additional information required by the referees forced us to both a reorganization and a
75 rewriting of this Section. The relevant changes are summarized here below:

- **We rewrite some parts of the Introduction to make the research objectives clearer**, as requested by Jana von Freyberg: “I think that the research objectives (or research questions) should be formulated more explicitly in the Introduction in order to guide the following analysis”.
- 80 - **We explain the difference between “direct input” and “delayed input”, previously addressed by von Freyberg et al. (2018), for estimating the F_{yw}^*** . In other words, we better underline the role of snowpack for estimating F_{yw}^* as requested by Jana von Freyberg: “The important aspect of snowpack 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...”.
- **We introduce the importance of the low-flow duration (LFD) and why it should affect (reduce) the F_{yw}^*** . We
85 have explained this aspect inserting the formulation of the flow-weighted average young water fraction (Eq. 1 of the revised manuscript), that is what we accurately predict using the amplitude ratio approach (Kirchner, 2016b).
- **We move a paragraph, explaining the importance of considering the role of Quaternary deposits for the reduction of F_{yw}^* at high elevations, from the Methods Section to the Introduction**. This was requested by referee #1: “L282-289: “Some authors have revealed ... F_{yw}^* in Alpine catchments”. This is more suitable for
90 Introduction than Methodology”.
- **We add a paragraph in which we clearly explain why we have not considered the mean annual precipitation as a possible explanatory variable for snow-dominated catchments**. This was a comment posted by Jana von Freyberg: “...What is the reason for not considering precipitation at all?”

1.4 Study sites: relevant changes

95 **The Study sites Section was completely reorganized and rewritten** following the comment of referee #1: "...The data description section (entire section 2) needs to be restructured and revised to make it more concise and clearer.". The relevant changes are summarized here below:

- **We describe the assembled data set and catchments attributes in a single Section (merging the “2.1 Existing data set”, “2.2 Additional data set” and “2.3 Complete data set” sections)**, as suggested by referee #1: “Both sections about the data (e.g., Section 2: existing data, additional dataset, complete data, and Section 3: discharge data and catchment boundary), why do the authors need two different sections?”. **A lot of information reported in the “old” 2.1, 2.2 and 2.3 sections have been inserted in the revised Supplementary Material.**
- **We insert the relevant information (elevation, slope, geology, average precipitation, discharge) about the study sites in a Table (Table 1 of the revised manuscript)** as suggested by referee #1: "...The data description section (entire section 2) needs to be restructured and revised to make it more concise and clearer. I think this can be done using a table.", "The authors can here focus more on catchment attributes, such as climate (e.g., average annual precipitation and discharge), land use cover, geology, and discharge." **In Table 1 we added a new column with the indication of the Period of isotope sampling (PoS) and we have updated the calculations so that precipitation and discharge are referred to the same time-window of isotope sampling**, as commented by Jana von Freyberg: "... 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} " and as requested by referee #1: "Line 190: I suggest mentioning the study period for the isotope data and F_{yw} for the different study catchments since it is different."
- **We insert Fig. 2 a,b and Fig. 2 c,d representing “the mean catchments slope against mean catchments elevation” and “the average precipitation and discharge against elevation”, respectively**, as requested by referee #1: "...I am curious to see the relation between average elevations and average slopes for the 27 catchments, is there a positive correlation? (Also, for average elevation with annual precipitation)". **Fig. 2a of the revised version corresponds to Fig. 4a of the submitted version. We move this figure to the Study sites Section** following the referee #1 suggestion: "Figure 4a can be moved to the data section...". **The figure is described and commented in the text.**
- **We insert Fig. 3 representing the boxplots of the mean monthly flow for the 27 study catchments according to their hydro-climatic regime.** In the submitted version all the boxplots were overlapped in a single figure (Fig.2 of the submitted version), while in the revised version we have separated the boxplots, according to the hydro-climatic regimes, as suggested by the referee #1: "...it is not easy to differentiate between the three boxplots, I would suggest having three separated boxplot figures with the same y axis limit...". **In addition, we better explain this figure in the text** following the referee #1 comment: "In summer there is a higher average monthly flow from snow-dominated catchments than from rain-dominated ones (due to increased snowmelt, I suppose), and in winter it

is the other way around. Please explain this better in the text because it is not clear... This figure should be described in the text (there is no description of this figure, it was only cited in line 243)". Describing the figure, **we add in the text information about discharge dynamics of our study catchments** as requested by the referee #1: "Here, I would expect more description of the discharge dynamics (e.g., giving an order of magnitude to these data by telling what the annual discharge is, whether the runoff is seasonal, etc)".

- **We modify Fig. 1 to improve the map visibility** according to referee #1 suggestion: "Figure 1: the background cannot be easily seen; I think you could replace with a DEM map. In addition, I cannot differentiate between Quaternary deposits and hybrid catchments visually."

135 1.5 Material and Methods: relevant changes

We have applied major changes to the Material and Methods Section. The main info presented in the submitted version remain in the revised Material and Methods Section but following the referees' comments/suggestions, we applied a reorganization of this Section. The relevant changes are summarized here below:

- **We remove the "3.1 Discharge data and catchments boundaries" sub-section**, as suggested by referee #1: "Much of the information provided in Study Sites, and Material and Methods is not relevant (e.g., shape file, detailed source of data, etc.). Instead, citing the sources of the various data (both from individuals and organizations) can be moved to either the Authors' Contributions or Acknowledgements, or in the supporting information Sections". **Accordingly, we move the content of this sub-section to the Data Availability section (at the end of the revised manuscript).**
- **We partially rewrite and integrate with additional info the old Section "3.2 Young water fraction estimation from seasonal cycles of stable water isotopes in precipitation and streamwater"**. In the revised manuscript, the content of this Section is reported in Section "3.1 Young water fraction estimation from seasonal cycles of stable water isotopes in precipitation and streamwater: the "direct" input.". We give here more details regarding the "direct input" approach (mentioned earlier in the Introduction) for the estimation of F^*_{yw} . In this regard, **we insert the Fig.4 of the revised manuscript (missing in the submitted version) for a visual representation of the "direct input" approach.**
- **We show here in Fig. 5 (i.e., Fig. 12 b of the submitted version) the relation between F^*_{yw} and mean catchments elevation.** Please note that the F^*_{yw} values are different from those used in the submitted version. **In the revised version, all the F^*_{yw} values are obtained with the "direct input" approach.**
- **We remove the Section "3.3 A new hydro-climatic regime classification: the classifiers"**. We decide to not introduce a new classification system as suggested by both the referees: "Why did the authors need to propose a new criterion for catchment classification? The authors used two variables: (1) streamflow ratio between different months and (2) snow cover fraction for the proposed catchment classification, but later they adjusted the threshold of these two variables to have consistent results with Staudinger et al. (2017). Why didn't they just use the method

160 of Staudinger et al. (2017)?”, “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.”
Following the referees’ comments, we decide to remove the new classification scheme, also because Jana von
Freyberg added: “Furthermore, the discussion of this new classification scheme (Sect. 4.2 and 5.1) somewhat
165 distracts from the main topic of the paper, which is the investigation of small F_{yw} in high-elevation catchments.”.
Accordingly, we classify Swiss catchments using the Staudinger et al. (2017) classification scheme, while we
classify Italian catchments using streamflow and topographical data, as suggested by Jana von Freyberg.
Specifically, we have found that Stoelzle et al. (2020) used a classification scheme (based on catchments elevation,
typical low-flow period, typical snow onset and typical begin of snowmelt) to classify catchments outside
170 Switzerland (e.g., German catchments).

- **We partially rewrite the Section “3.4 Average fractional Snow Cover Area (F_{SCA}) computation”. This is
because we revised the methodology for estimating the f_{SCA} since the old methodology was prone to an
overestimation of f_{SCA} as noticed by the referees: “Eq: (5) the denominator ($N_{tot} - N_{clouds}$): This could result in an
overestimation of f_{SCA} .”, “Can it be that f_{SCA} tends to be over-estimated with your approach?”. **If $f_{SCA} > 1$ we
175 calculate f_{SCA} as N_{snow} / N_{tot} since this is the only heuristic solution that guarantees no overestimation.** The effect
of the new methodology can be seen comparing Fig.13a of the revised manuscript with Fig. 12a of the submitted
version. **The revised Section title is “3.2 Snow cover persistence quantified through the average fractional
snow cover area (F_{SCA})”****
 - **We partially rewrite the Section “3.5 Accounting for groundwater: fraction of quaternary deposits (F_{qd}),
180 Winter Flow Index (WFI) and baseflow fraction (F_{bf})”. In the revised version the section title is “3.3 Fraction
of Quaternary deposits, low-flow duration and the groundwater contribution to the stream”. We added more
details about the baseflow separation of Duncan (2019) and **we add here a paragraph in which we define the low
flow period (T_{Low}) and the low flow duration (LFD). We also add a paragraph in which we explain how we
estimate the uncertainty of F_{bf} (it was not computed in the submitted version).****
 - **We insert Fig. 6 to show the coverage of Quaternary deposits for some representative study catchments.**
- 185

1.6 Results and Discussion: relevant changes

Please note that we change paper structure from “Results/Discussion” to “Results and Discussion”. The main
difference is that in the submitted version we first present the “objective” results and then we comment on such results in the
Discussion section. In the revised version results and comments are coupled and merged in a unique Section. Some major
190 and minor comments of referee #1 have led us to the use of the “Results and Discussion” structure: e.g., “The manuscript
needs to be restructured and revised. There is a lack of clarification in the text.”, “L389-391: Our results ... for hybrid
catchments (median F_{yw} of 0.32) ... Why are there these differences? I suggest arguing and explaining them.”. In this second

comment referee #1 asked us to argue about the results, probably because she/he expected to read a Discussion coupled with the result. In fact, we have accounted for an explanation of the differences required by the referee #1 in the Discussion section. For these reasons, we realise that a “Results and Discussion” structure could improve the readability and make the text more fluent.

Having said that, the relevant changes are summarized here below:

- **We move the sub-section “4.1 Young water fraction estimation for DOR and SOU catchments” to the Supplementary Material**, as suggested by referee #1: “Section 4.1. I think this can be moved to the data section or supporting information, as this is only for 2 catchments.”
- **We remove the subsection “4.2 The new hydro-climatic regime classification”**. This is because we have decided to remove the new classification scheme from the paper and to use the method of Stoelzle et al. (2020) to classify catchments outside Switzerland.
- **We remove the sub-section “4.3 New explanatory variables for the F_{yw} elevation gradients”**. **Some information of this sub-section have been moved to the Study sites section**, as suggested by referee #1: “Figure 4a can be moved to the data section...” (we move the figure and its description). We remove Fig. 4b and its description since the statements expressed in the submitted version were not well supported according to Jana von Freyberg: “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)”.
- **We modify the order of the results presentation from “4.3.1 Fractional Snow Cover Area (f_{SCA}) and F_{yw} ”, “4.3.2 The role of quaternary deposits”, “4.3.3 Groundwater contribution to streamflow: WFI and F_{bf} related with F_{yw} ” to “4.1. The role of Quaternary deposits”, “4.2 Stored (old) water contribution to streamflow (F_{bf}) and F^*_{yw} ”, “4.3 Low-flow duration (LFD) and F^*_{yw} ”, “4.4 The role of snowpack persistence”**
- **We remove Fig. 8a and move Fig. 8b to the Supplementary Material**. This is because the WFI somewhat deviates from the fil rouge of the paper.
- **We update the Fig. 7 (in the revised version Fig. 7a) with the F^*_{yw} obtained through the “direct-input” approach and we underline the negative correlation between F^*_{yw} and F_{qd} found for the snow-dominated catchments.**
- **We add the Fig. 7b representing the “Fraction of quaternary deposits against mean catchments elevation”** following the suggestion of referee #1: “The objective is to investigate what drives F_{yw} variation with elevation. The authors proposed using a new set of hydrological variables, but what are the relations between these variables with elevation? For example, what are the relations between F_{SCA} , F_{qd} , F_{bf} with elevation?”. Accordingly, **we comment about the Fig. 7b in the text.**
- **We add a new sub-section “4.2.1 The complementarity between the fraction of baseflow (F_{bf}) and the young water fraction (F^*_{yw})**. We do this because we want to stress more the novelty of this result as suggested by Jana

von Freyberg: "...I would like to encourage the authors to highlight more the novelty of their findings and the scientific contribution of their work".

- **We update the old Fig. 10 with Fig. 9a of the revised manuscript.** In fact, we plot the F_{yw}^* obtained through the "direct-input" approach against the F_{bf} . We show that the linear fit of these data is really close to the complementary line.
- **We add Fig. 9b to better show the complementarity of F_{yw}^* and F_{bf} both varying with elevation,** as requested by referee #1: "The objective is to investigate what drives F_{yw} variation with elevation. The authors proposed using a new set of hydrological variables, but what are the relations between these variables with elevation? For example, what are the relations between F_{SCA} , F_{qd} , F_{bf} with elevation?"
- **We add the sub-section "4.3 Low-flow duration (LFD) and F_{yw}^* ".** In this Section we discuss the relation between F_{yw}^* and LFD (Fig. 11a) and the LFD variations with elevation (Fig. 11b). We also insert the Fig. 10 showing the relation between LFD and F_{bf} .
- **We rewrite section "4.3.1 Fractional Snow Cover Area (f_{SCA}) and F_{yw} " since we have obtained new results (please note that both F_{yw}^* and F_{SCA} have been calculated in a different way with respect to the submitted version). Accordingly, we update the old Fig. 5 (Fig. 12 of the revised version) and the old Fig. 12a (Fig. 13a of the revised version) with the new values.**
- **We add the Fig. 13b representing the " F_{SCA} against mean elevation"** following the suggestion of referee #1: "The objective is to investigate what drives F_{yw} variation with elevation. The authors proposed using a new set of hydrological variables, but what are the relations between these variables with elevation? For example, what are the relations between F_{SCA} , F_{qd} , F_{bf} with elevation?" Accordingly, **we comment about the Fig. 13b in the text.**
- **We add the Fig. 14 representing the "LFD against F_{SCA} " and we comment about it in the text.**
- **We add a paragraph discussing thoroughly the results obtained for the NBPV (glacier-dominated) catchment** since it was requested by referee #1: "L518: I suppose the fast flow paths are due to the fact that the glacier acts as an impermeable layer and thus promotes rapid overland flow? Please explain what you mean."
- **We add Table 2 reporting F_{yw}^* , F_{qd} , LFD , F_{bf} , F_{SCA} and WFI values for all the study catchments.**
- **We add a new paragraph "4.5 Process interplay along elevation: perceptual model"** explaining the perceptual model.
- **We improve the old Fig.13 (representing the perceptual model) with Fig.15 of the revised manuscript.** We modify the figure according to the Jana von Freyberg comment: 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?"

1.7 Conclusion: relevant changes

We reformulate the Conclusion to better communicate the novelty of our results in a synthetic and effective way. Consequently, we rewrite and reorganize this Section. This is a direct consequence of the major changes applied at the whole manuscript.

- 265 - We remove from the Conclusion the paragraphs related to the new classification scheme (no more considered in the revised paper) and related to the Winter Flow Index.
- We clearly communicate the role of the low flow duration in reducing the F_{yw}^* (this was missing in the submitted version)
- We add future challenges such as the collection of detailed geological information, the collection of isotopic data
270 from glacier-dominated systems (which hydrological processes are still poorly understood), the development of new automated techniques to improve the modelling of the groundwater contribution to the stream.

2 Response to Referees

2.1 Response to referee #1

*The work of Gentile et al. investigated the causes for young water fraction (F_{yw}) variations with elevation (F_{yw} is low at high
275 altitudes) in Alpine catchments. The study areas are 27 catchments in Switzerland and Italy. The authors proposed new criteria for catchment classification into different hydro-climatic regimes. To gain insight into the reason for F_{yw} variations with elevation, this author used a new set of hydrological variables, namely the fractional snow cover area (F_{SCA}), the fraction of quaternary deposits (F_{qd}), and the fraction of baseflow (F_{bf}). In general, the idea of this paper about what drives F_{yw} variations with elevations is novel and of interest for understanding the functioning of catchments in Alpine regions as
280 well as for understanding flow and transport in this region and potentially in other areas. However, the methodology and results do not fully support this idea. The text was not well written. Please find my main comments and line-by-line comments below.*

Dear referee #1,

285 We would like to thank you for the overall positive assessment and the numerous detailed comments, which contributed to our manuscript's improvement considerably.

Please find below a point-by-point response to both your main and minor comments. We have incorporated all your constructive feedback in the revised manuscript.

290 Sincerely,

The Authors

2.1.1 Main comments

295 1) *Why did the authors need to propose a new criterion for catchment classification? The authors used two variables: (1) streamflow ratio between different months and (2) snow cover fraction for the proposed catchment classification, but later they adjusted the threshold of these two variables to have consistent results with Staudinger et al. (2017). Why didn't they just use the method of Staudinger et al. (2017)?*

300 **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 the Staudinger et al. (2017) classification scheme cannot be strictly applied since they were designed for the Swiss hydro-climatic regimes. We have “manually calibrated” 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 catchments studied by Staudinger et al. (2017) and we can apply it also outside the Swiss borders.**

305 **However, according to the referees' comments, we have removed the new criterion for catchment classification from the revised version of the manuscript. We use the classification scheme of Stoelzle et al. (2020) which is based on catchments elevation, typical low-flow period, typical snow onset and typical begin of snowmelt. This scheme was used in the past by the authors to classify catchments outside Switzerland (i.e., German catchments).**

310 2) *The objective is to investigate what drives F_{yw} variation with elevation. The authors proposed using a new set of hydrological variables, but what are the relations between these variables with elevation? For example, what are the relations between F_{SCA} , F_{qd} , F_{bf} with elevation? With F_{SCA} , I can infer from the text, but it was not explained in the text until the last sections (Section 5.2) of the manuscript. F_{SCA} cannot be directly related to elevation, instead, it needs to be related to the catchment classification then from catchment classification to mean elevation. However, in other areas, can we still relate F_{SCA} to elevation? With the other variables (F_{qd} and F_{bf}), it is unclear to me what are their relations to elevations. In addition, F_{qd} does not seem to be a good variable because there is no significant relation between F_{yw} and F_{qd} .*

320 **We added in the revised manuscript, for each variable (F_{SCA} , F_{qd} and F_{bf}) a figure (Fig. 13b, Fig. 7b, Fig. 9b, respectively) that shows the relation with mean catchments elevation.**

- 325 a) The F_{SCA} increases with the mean catchment elevation in our data set, revealing a positive, statistically significant correlation. This suggests the increasing snowpack persistence with elevation. See Section 4.4 of the revised manuscript.
- 330 b) F_{qd} decreases with the mean catchment elevation in our data set, revealing a negative, statistically significant correlation. This negative correlation reflects the fact that F_{qd} decreases when the mean slope increases (Arnoux et al., 2021) (mean slope increases with mean elevation for the catchments analyzed in this study, as shown in Fig. 2a of the revised manuscript). We use F_{qd} because Arnoux et al. (2021) demonstrated a strong positive correlation between F_{qd} and Winter Flow Index (WFI) highlighting the role of unconsolidated deposits in storing groundwater since this low-flow indicator reflects the groundwater (in terms of age, old water) contribution to the stream (Arnoux et al., 2021; Cochand et al., 2019; Paznekas and Hayashi, 2016). The missing information about the portion of fractured bedrocks, the thickness of Quaternary deposits and the bedrock topography will demand future attention for a complete picture of the role of geology (potentially resulting in a statistically significant correlation with F^*_{yw}). See Section 4.1 of the revised manuscript.
- 335 c) F_{bf} against elevation reveals an opposite (and complementary) behavior with respect to F^*_{yw} : it decreases until 1500 m a.s.l. and it increases at higher elevations (Fig. 9b of the revised manuscript). This complementarity is an important result for catchments where isotope measurements are missing. In such catchments, the F^*_{yw} could be potentially estimated without the application of the amplitude ratio approach as: $F^*_{yw} \simeq 1 - F_{bf}$.
- 340
- 345 3) *The manuscript needs to be restructured and revised. There is a lack of clarification in the text. More description of the study area characteristics is needed. Much of the information provided in Study Sites, and Material and Methods is not relevant (e.g., shape file, detailed source of data, etc.). Instead, citing the sources of the various data (both from individuals and organizations) can be moved to either the Authors' Contributions or Acknowledgements, or in the supporting information Sections or to a table rather than describe them within the text of the article, making it very difficult to read such detailed information. If possible, I would also suggest the authors publish their data in an open repository.*
- 350

The whole manuscript has been restructured and revised (See Section 1 of this document to see all the details about the relevant changes). We revised both the fil rouge that allows the reader to follow our reasoning and the language, thus improving the text clarification. As reported in Section 1 of this document, the Study sites Section was completely reorganized and rewritten, and all the data sources have been moved to the *Data availability* Section (at the end of the manuscript), as you have suggested.

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2.1.2 Minor comments

- 1) Title: “ F_{yw} ” could be changed to “young water fraction” for general readers.

We change the title from: “What drives F_{yw} variations with elevation in Alpine catchments?” to “Towards a conceptualization of the hydrological processes behind changes of young water fraction with elevation: a focus on mountainous alpine catchments.” We have changed “ F_{yw} ” to “young water fraction” for general readers.

- 2) L14: “The young water fraction (F_{yw}),..., is increasingly used in hydrological studies, replacing the widely used Mean Transit Time, which is subject to aggregation error.” This sentence provides misleading information. I think F_{yw} cannot replace Mean Transit Times (MTT) since the two characterize different aspects of the transit times, e.g., F_{yw} contains information about the younger part of the TT distribution (how much water in outflow is younger than 0.2 years) while MTT contains information about the whole TT domain. “Aggregation error” could be changed to “aggregation bias”.

Thanks. Our statement was indeed not precise. We wanted to say that, before the work of Kirchner (2016a, b), the Mean Transit Time (MTT), obtained with convolution, was used in catchment intercomparison studies. After that key paper, it is generally replaced with F^*_{yw} that, of course, does not provide the same information as MTT . To avoid misunderstanding, we change the sentence simply by writing: “The young water fraction (F^*_{yw}), defined as the fraction of catchment outflow with transit times of less than 2-3 months, is increasingly used in hydrological studies that exploit the potential of isotope tracers.”

- 3) L33-34: *The sentence “... F_{bf} , considered...complement of F_{yw} ” does not clearly show the relation you found between F_{yw} and the baseflow fraction. Please be clearer about what you mean by explicitly saying that F_{bf} is a good proxy for F_{yw} as the higher F_{yw} is, the lower F_{bf} .*

Thank you. In the revised version we have dedicated a whole sub-section to the (complementary) relation between F^*_{yw} and F_{bf} in which we clearly express the negative correlation between F^*_{yw} and F_{bf} , also indicated by the linear fit of the data. For more details, please see Section 4.2.1 of the revised version. In the abstract we write: “In our data set F_{bf} reveals a strong complementarity with F^*_{yw} , suggesting that the latter could be estimated as $F^*_{yw} \approx 1 - F_{bf}$ for catchments in which stable water isotopes measurements are not available.”

- 4) L44: *“the streamflow is older than the annual snowmelt” is not clear to me, what is the age of streamflow and the age of snowmelt water in this case?*

We have clarified this in the revised version. From Introduction: “An early work in the Swiss Alps shows that high celerity is caused by massive meltwater infiltration that pushes out groundwater reserves: streamflow following snowmelt is older than meltwater infiltrated in the current year (Martinec, 1975)”

- 5) L46: why “even”? I would expect exactly that during the absence of rainfall and snowmelt the streamflow is mainly sustained by groundwater.

390

We wanted to underline that the hydrograph separation results show that the hydrograph is generally mainly composed of old water at the peak flow. Of course, during no-rain and no-snowmelt periods we expect that streamflow is mainly sustained by groundwater and this is also confirmed by our results. However, we remove this sentence from Introduction.

- 6) L46-50: The two sentences here do not seem to be connected, one about residence time and the next one about transit times.

395

Of course, the transit time distribution and the residence time distribution are two separate concepts. Nevertheless, the streamwater age is influenced by the storage age depending on how much the storage contributes to the stream. To avoid confusion, we remove the second sentence.

- 7) L53: “Kirchner (2016a, b) proposed a new metric to quantify water age at the catchment scale”. I think you are mentioning the F_{yw} , I don’t think this is “the water age at the catchment scale” but the amount of water with age < 0.2 years. How can we know the “water age at the catchment scale “only based on the amount of water in outflow (discharge) that is < 0.2 years? For example, if $F_{yw} = 0.2$, what is the “water age at the catchment scale”

400

We wanted to say that MTTs, obtained with the classic convolution approach, are no longer used since they are subject to the aggregation bias. However, we can say something reliable about water age using a new metric: the young water fraction, that is calculated at the catchment scale. Of course, F^*_{yw} is not giving the same information of MTT. We have rephrased the sentence: “Kirchner (2016a, b) proposed a new metric to quantify the share of catchment outflow with transit times lower than roughly 0.2 years or 2-3 months: the young water fraction.”

405

- 8) L55-58: please revised the sentence structure

410

We have revised the sentence: “ F^*_{yw} is increasingly used in hydrological studies because it has the advantage of being free from the aggregation errors inherent to Mean Transit Time (MTT) estimates obtained through the classical convolution approach (Kirchner, 2016a)”.

- 9) L58-59: please see my comments on line 14

415

We have rewritten: “Even more so, F^*_{yw} is an informative descriptor of catchment hydrological functions, of nutrients cycles and of pollutant transport (Stockinger et al., 2019; Benettin et al., 2017; Jasechko et al., 2016)”

- 10) L70: “In line with these findings” can be removed because Lutz et al. (2018) did not state that F_{yw} above 1500 m decreases

420

Yes, you are right. Lutz et al. (2018) did not state that F^*_{yw} decreases above 1500 m a.s.l., but they said "In agreement with the results from the global study of European catchments, there is a slight tendency toward smaller F_{yw} values for the subcatchments in the mountainous region". Therefore, to be more precise, we

write “Interestingly, in their data set, a statistically significant positive correlation with elevation was obtained after removing from their analysis the five snow-dominated catchments, which revealed the smallest F^*_{yw} values (von Freyberg et al., 2018). Likewise, Lutz et al. (2018) estimated F^*_{yw} for 24 catchments in Germany and found the smallest values for higher-elevation sites.”

425 11) L82-83: “...more efficient groundwater recharge, consequently reducing or increasing the young streamflow...” It is not clear to me, should it be “reducing” only instead of “reducing or increasing”?

430 Ceperley et al. (2020) said: "our highest elevation study site (NBPV) deviates from this trend by yielding a higher F_{yw} , it is too early to draw the conclusion that low F_{yw} could be due to seasonal versus intermittent snow cover dynamics alone." So, in the submitted version, we remain vague in the Introduction saying "reducing or increasing". From our results presented at the end of the manuscript, we can say that seasonal snow cover favors the groundwater storage emptying during winter and the groundwater storage recharge (because of meltwater infiltration) during summer, thus reducing the young streamflow reaching the stream. In the revised Introduction we write: “However, it is still unclear if seasonal or ephemeral snow cover dynamics can affect the F^*_{yw} (Ceperley et al., 2020)”.

435 12) L88: “...remarkable fraction of groundwater...” it is a bit vague, could you please be more precise?

To be more precise, we have specified the average percentages of groundwater according to the cited works: “Several studies located in the Rocky Mountains and Andes show that, on average, about 47% of groundwater annually sustains the streamflow (Saberli et al., 2019; Somers et al., 2019; Carroll et al., 2018; Harrington et al., 2018; Cowie et al., 2017; Baraer et al., 2015; Gordon et al., 2015; Frisbee et al., 2011; Liu et al., 2004; Clow et al., 2003; Baraer et al., 2009). Similar results are also found in the Himalayas (49%) and the Alps (48%) (Chen et al., 2018; Engel et al., 2016; Käser and Hunkeler, 2016; Williams et al., 2016; Wilson et al., 2016; Andermann et al., 2012)”.

440 13) L91-92: “...a dynamic storage contribution to streamflow...” Please clarify this term.

445 With “dynamic storage”, we refer to the storage that controls the streamflow dynamics (Staudinger et al., 2017). However, we remove this sentence from the revised version.

450 14) L99: Why don't the authors use a new set of hydrological variables (F_{SCA} , F_{qd} , F_{bf} , WFI) in combination with traditional variables to gain new insights into the F_{yw} along elevation gradients?

We do not know what is meant by "traditional" variables. However, we decided to use variables that were not previously considered for explaining F^*_{yw} elevation gradients. This is also because 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. We observe no significant correlation between the young streamflow fraction and catchment size, annual precipitation, bedrock porosity, population density, or the fraction of catchment area comprised of pasture land or open water". We specify this in the Introduction of the revised version.

15) L104: “...into three hydro-climatic regimes proposing a new criterion of classification...” Why? I think a brief explanation is needed.

Please see our answer to your first main comment.

460 16) Sections 2 and 3.1: Both sections about the data (e.g., Section 2: existing data, additional dataset, complete data, and Section 3: discharge data and catchment boundary), why do the authors need two different sections? The data description section (entire section 2) needs to be restructured and revised to make it more concise and clearer. I think this can be done using a table. In the text, the authors could summarize and report key information, so the reader does not have to search through the many sources you have cited. The authors can here focus more on catchment attributes, such as climate (e.g., average annual precipitation and discharge), land use cover, geology, and discharge.

465 **Thank you. We apply the suggested structure that improved the paper readability. We have completely restructured Section 2 according to your comments. We have condensed “existing data, additional dataset and complete data” Sections in a single Section (Section 2 of the revised manuscript). Moreover, we have moved all the data sources, reported in Section 3 of submitted version, to the “Data availability” Section of the revised version. We have improved the study sites description summarizing the main topographic and hydro-climatic quantities in a Table (Table 1 of the revised version) and some figures (Fig.2 and Fig.3 of the revised version).**

470 17) “Furthermore, 21 out of the 22 ... (Staudinger et al., 2017)”. This part is not relevant in my opinion.

You are right. We removed this part in the revised version.

475 18) Two high-elevation catchments ... Arnoux et al., 2021)”. This part is not relevant in my opinion.

You are right. We removed this part in the revised version.

19) L147: In my opinion, the “Complete Dataset” subsection is not necessary. It is sufficient to illustrate the existing data in subsection 2.1 and conclude the section with 2.2.

Thank you. we removed the “Complete dataset” subsection as suggested.

480 20) L156-160: Like von Freyberg (2018) ... are reported in Table 1. If subsection 2.3 is deleted, move it to 2.2 as the final sentence.

485 **We have completely restructured the Section 2. The paragraph about the hydro-climatic classification is no more at the end of the Section. We have written: “In order to be consistent with previous studies (von Freyberg et al., 2018; Staudinger et al., 2017), we classify the 23 Swiss catchments according to the hydro-climatic regimes proposed by Staudinger et al. (2017) which group the regimes defined by Weingartner and Aschwanden (1992) in three categories: rainfall-dominated (R), hybrid (H) and snow-dominated (S). For the four Italian catchments, where the aforementioned classification schemes cannot be rigorously applied, we use that proposed by Stoelzle et al. (2020).”**

21) *Figure 1: the background cannot be easily seen; I think you could replace with a DEM map. In addition, I cannot differentiate between Quaternary deposits and hybrid catchments visually.*

490

We have changed Fig.1 according to your suggestions (see new Fig. 1 of the revised manuscript). We do not show the quaternary deposits cover in Fig. 1. This is shown for some representative study catchments in Fig. 6 of the revised manuscript.

495

22) *Table 1: I am curious to see the relation between average elevations and average slopes for the 27 catchments, is there a positive correlation? (also for average elevation with annual precipitation).*

We have inserted these two figures in the “Study sites” section. We have commented in the revised version: “The average slope ranges from 4° to 34°, and our study sites reveal an increase of steepness with elevation (Fig. 2a, Fig.2b). Precipitation increases with elevation until 1500 m a.s.l. and it decreases for higher elevations (Fig. 2c, Fig. 2d), highlighting a change of precipitation regime as described by previous studies (Santos et al., 2018).”

500

23) *Section 3.1: Here, I would expect more description of the discharge dynamics (e.g., giving an order of magnitude to these data by telling what is the annual discharge, whether the runoff is seasonal, etc). I would suggest moving the description of how discharge was measured and derived to the appendix. The source of data could be combined into the same table suggested for section 2 (or move to the appendix or data availability section).*

505

Thank you for these suggestions. In the revised version we have moved all the info about the data sources to the “Data availability” Section, while we have described discharge dynamics in the “Study sites” Section.

24) *Line 190: I suggest mentioning the study period for the isotope data and F_{yw} for the different study catchments since it is different.*

510

Thank you for this suggestion. In the revised version we have reported this info in the last column of Table 1 (“Study sites” Section).

25) *Figure 2: In summer there is a higher average monthly flow from snow-dominated catchments than from rain-dominated ones (due to increased snowmelt, I suppose), and in winter it is the other way around. Please explain this better in the text because it is not clear. In addition, it is not easy to differentiate between the three boxplots, I would suggest having three separated boxplot figures with the same y axis limit. This figure should be described in the text (there is no description of this figure, it was only cited in line 243)*

515

We have subdivided the figure in three separated boxplot (see Fig. 3 of the revised version) and we have explained it better in the “Study sites” Section: “Across the three considered streamflow regimes, a shift of the monthly hydrograph peak (computed using discharge data in the PoS) from winter to summer months is observed (Fig. 3): this “flow peak-shifting” is a clear sign of the increasing predominance of snowmelt in the streamflow generation processes.”.

520

26) L197: no comma after “where”

Thank you.

27) L221: As I understood from the text (before and after this line), there is indeed a “formal” classification method

525 **Yes, there is a formal classification method proposed by Weingartner and Aschwanden (1992), but it was designed for Switzerland. The regimes defined by Weingartner and Aschwanden (1992) were grouped by Staudinger et al. (2017) in three categories: rainfall-dominated, hybrid and snow-dominated. Searching in scientific literature, after the interactive discussion phase, we have found that Stoelzle et al. (2020) proposed a classification method based on topographical and streamflow data and that this method was used in the**
530 **past to classify catchments outside Switzerland. Thus, we decide to use this formal classification method to classify the Italian catchments of our dataset.**

28) *Section 3.3: After reading the entire manuscript up to section 3.3. I am not clear why the authors need to classify streamflow into three regimes and why the classifier should be based on snow-related characteristics (e.g., snow cover area).*

535 **Please see our answer to your first main comment.**

29) *L240: should it be “it is expressed in mm per unit area and time step”?*

We have removed the Section “3.3 A new hydro-climatic regime classification: the classifiers” since we use literature classification schemes.

30) *L251: “...more than weekly...” do you mean biweekly?*

540 **We have rewritten: “Temporally, this relatively recent satellite has increased the visitation frequency to a sub-weekly temporal resolution and increased the spatial resolution to 20 m for snow cover (Gascoin et al., 2019).”**

31) *Eq: (5) the denominator ($N_{tot} - N_{clouds}$): This could result in an overestimation of f_{SCA} . What is the maximum fraction of cloud cover in these images?*

545 **We follow the approach of Hofmeister et al. (2022) and Di Marco et al. (2020) that define f_{SCA} as $N_{snow}/(N_{tot} - N_{clouds})$. They did not comment on the effect (i.e., underestimation or overestimation) of the mathematical expression of f_{SCA} on their results. If the two detection algorithms (snow detection and cloud detection) would work with a 100% accuracy, values greater than 1 cannot be encountered. In fact, the maximum cloud cover fraction can also be very close to 1 in some dates (e.g., > 90% as encountered in our data set), but if the snow**
550 **detection algorithm works well N_{snow} will be at most “complementary” to N_{clouds} (i.e., $N_{snow} + N_{clouds} = N_{tot}$) and f_{SCA} will be at most 1. Sometimes these algorithms can result in a misclassification of pixels and f_{SCA} values > 1 can be encountered: i.e., $N_{snow} > (N_{tot} - N_{clouds})$. Our approach was to set $f_{SCA} = 1$ if $f_{SCA} > 1$. We deepen that this approach can overestimate the f_{SCA} . Thus, we propose a new approach: if $f_{SCA} > 1$ we calculate f_{SCA} as N_{snow}/N_{tot} since this is the only heuristic solution that guarantees no overestimation. Please see Section 3.2 of the**
555 **revised version for more details.**

32) L276-279: The error in f_{SCA} is still there with the “moving window” approach, it is just smoothed. Anyways, at the end, you calculated the average f_{SCA} over the whole period so applying “moving average on a window” does not have any effect?

560 **Of course, the average of the series after the application of the moving average (that is what we call F_{SCA} in the submitted version) is not the same as the average over the original series. However, in the revised manuscript we change the Methodology and we do not apply the moving average. Please see Section 3.2 of the revised version for more details.**

33) L282-289: “Some authors have revealed ... F_{yw} in Alpine catchments”. This is more suitable for Introduction than Methodology. In addition, what is “key possibility”? Does it mean “high possibility”

565 **Thank you. We have moved this part to the Introduction. With “key possibility” we wanted to express “the importance” of Quaternary deposits (moraines, alluvium, and talus) in storing groundwater. We have simply written in the Introduction: “Some authors have revealed the possibility of quaternary deposits (e.g., talus, moraine, alluvium) to store groundwater in high-elevation alpine catchments (Arnoux et al., 2021; Hayashi, 2020; Christensen et al., 2020).”**

34) L292: ... 23 Swiss catchments ... Is F_{qd} calculated only for 23 sub-catchments, while WFI and F_{bf} for all 27? Why? How does it affect the interpretation of the results? Be clearer about which indices are available for each study site.

570 **We have written: “Operatively, for the 23 Swiss catchments of our dataset, we calculate the portion of the catchment area occupied by Quaternary deposits using the Geological Atlas of Switzerland...” The number “23” refers to catchments located in Switzerland because, for these catchments, we have used the GeoCover dataset for estimating the area covered by Quaternary deposits; for the remaining four Italian catchments we have used regional geological dataset. F_{qd} , WFI and F_{bf} are calculated for all the 27 catchments: see also Table 2 of the revised manuscript.**

35) L299-301: For the DOR and SOU ... provided by Dr. Giulia Zuecco. This part is not relevant here, should be moved to the data section.

580 **We have moved this part to the Data availability section as you have suggested.**

36) L315-318: “For VdN, NBPV and BCC catchments we consider the time windows ... we consider discharge measurements in the period November 2017 - January 2022”. I think you should indicate at the beginning the different study periods, because it is confusing to read a lot of data (e.g., stable isotopes of water, F_{yw} , streamflow...) and indices (e.g., F_{qd} , WFI , F_{bf}) for your methodology and find out that your study areas were analyzed in different periods. You should say this explicitly each time you mention a new data item or index or create a table in which you explain it.

585 **Thank you for this recommendation. We have inserted this info in Table 1 reported in the “Study sites” Section of the revised version.**

590 37) Section 4.1. I think this can be moved to the data section or supporting information, as this is only for 2 catchments.

Thank you for this suggestion. We have moved this subsection to the Supplementary Material.

38) L334-335: “these have the same names as the ones proposed by Staudinger et al. (2017 but the classification is not based on the same criteria” why? I think should be explained earlier in the methodology section.

595 39) L336-337: “In order to achieve a classification as consistent as possible with that of Staudinger et al. (2017), but based on these two variables, we propose the thresholds presented in Table 2:” I cannot understand why. If the authors want to have consistent results with Staudinger et al. (2017), why did not they use the method proposed by Staudinger et al. (2017)?

Regarding comments 38 and 39, please see our answer to your first main comment.

600 40) L345-346: “Following our classification scheme, ... and 9 snow-dominated catchments”. How do you compensate for the fact that the catchments data belong to different periods?

41) L353: “snow-regime” should be explained here

42) L354: “for the first order estimate of the second classifier” what does it mean?

Regarding comments 40,41 and 42, we have removed Section “4.2 The new hydro-climatic regime classification” since we use literature classification schemes.

605

43) “Section 4.3: New explanatory variables for the F_{yw} elevation gradients” I would expect all subsections in section 4.3 will use variables that are related to elevation to explain the relation between F_{yw} with elevation. However, I cannot see what is the relation between the variables in the section title (e.g., Section 4.3.1. Fractional Snow Cover Area (f_{SCA}) and F_{yw}) and elevation (Please also see my main comments)

610

Thank you for this comment. Following this comment, we have related all the variables with elevation and all the new figures are explained in the text. Please see in the “4 Results and Discussion – Towards a harmonious and exhaustive framework of the hydrological processes that drive the young water fraction variations with elevation.” Section of the revised manuscript Fig. 7b, Fig. 9b, Fig. 11b, Fig. 13b.

44) L361-368: part of this information was already described in the introduction, can be removed here or merged into the introduction.

615

Thank you for this suggestion. We have merged this part in the Introduction: “These results are partially consistent with those of Jasechko et al. (2016): based on the analysis of 254 watersheds worldwide, their work revealed a reduction of F^*_{yw} in mountainous, steeper terrains. This could be related to deep vertical infiltration caused by fractures generated by high rock stress in complex terrain morphologies or by freely draining soils (i.e., cambisols and luvisols), both associated to high-elevation environments (Lutz et al., 2018; Jasechko et al., 2016; Gleeson et al., 2014). In addition, the higher the topographic roughness is, the longer are the flow paths, with a consequent rise of transit times (Gleeson and Manning, 2008; Frisbee et al., 2011; Jasechko et al., 2016).”

620

625 45) L389-391: *Our results ... for hybrid catchments (median F_{yw} of 0.32) ... Why are there these differences? I suggest arguing and explaining them.*

Thank you. We have explained the differences in F_{yw}^* values with varying hydro-climatic regimes in Sections 4.2, 4.3 and 4.4 of the revised manuscript.

630 46) Figure 4a can be moved to the data section, figure caption: “the horizontal bars correspond to +/- standard deviation” of slope or elevation?

Thank you for the suggestion. We have moved the figure to the “Study sites” section (see Fig. 2a of the revised manuscript), but we have inverted the axes. The horizontal bars of the “old” Fig. 4a correspond to +/- standard deviation of slope. In Fig. 2a of the revised manuscript the standard deviation of slope is represented by the vertical bars.

635 47) L367: *“Despite this” why should an increase in slope with elevation result in a correlation between F_{yw} and slope?*

Thank you for this comment. This is probably a typo. We could expect a negative correlation between F_{yw}^* and slope because of the results of Jasechko et al. (2016): “...topographic gradient provides the strongest (negative) correlation with young streamflow fractions in our data set...”

640 48) L393: “lowering” could be changed to “decreasing of F_{yw} with increasing F_{SCA} ”

Thank you.

645 49) L484-486: Therefore, it is more likely that ... possibly ephemeral, snowpack. I do not see a connection between these two statements. If you are saying that lower-order (i.e., more downstream) channels release greater amounts of old water than higher-order (i.e., more upstream) channels, why do you say that water age decreases with elevation? Please clarify this point.

We have clarified this point in Section 4.4 of the revised manuscript. L484-486 of the preprint refer to the discussion about the rising limb (when F_{SCA} remains below roughly 0.3) of the F_{yw}^* vs F_{SCA} bell-shaped relationship (see Fig. 13a of the revised manuscript). In other words, for rainfall-dominated and hybrid catchments with ephemeral snowpack, F_{yw}^* increases with elevation because:

- **The increase of precipitation with elevation and the reduction of evaporation with elevation, due to reduced temperatures, promote wetness conditions that increase F_{yw}^* .**
- 650 - **The limited number of snowfall days and the mid-winter melt (due to an ephemeral snowpack) reduce the snow accumulation. Such a snowpack does not protect the underlying soils from freezing thereby inhibiting infiltration and favoring rapid flow paths during mid-winter melt/rainfall events, with subsequent increase of F_{yw}^* .**
- 655 - **These short-lived snowpacks melt during the winter season resulting in little delay between precipitation input and melt (i.e., no water aging in the snowpack).**
- **Considering the Strahler’s stream order, lower order channels, upstream, are more rarely activated (e.g., because of intense rainfall/snowmelt events) draining young water. Vice versa, higher order channels,**

downstream, are more often active (e.g., because of low/medium rainfall/snowmelt events) and inclined to drain more old water. Please note that we have removed this last point from the paper because we do not want to generate confusion: we are studying F^*_{yw} with elevation among different catchments and not within a single catchment. Probably, considerations about the age of water flowing in higher/lower order channels is more suitable if looking for water age variations within a single catchment.

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665
50) L493: “a persistent, deep snowpack can promote deep vertical infiltration by insulating the soil and thereby preventing freezing” do you mean this happens in winter? If in winter, there might be only snow, how can it be melted and promote deep vertical infiltration? Where is the source of water for vertical infiltration?

The persistent and deep snowpack prevents soil freezing during winter so that during snowmelt onset in spring, meltwater can infiltrate and recharge the groundwater storage. We clarify this in Section 4.4 of the revised manuscript.

670
51) L495: what’s a temporal concentration? Make it clearer.

We have written: “The resulting effect on water partitioning between the surface and the subsurface should be analyzed considering the temporal concentration of water input on the snowmelt period, but this remains largely unexplored to date (Rey et al., 2021)”

Temporal concentration: the time-interval in which the snowmelt enters the system as water input.

675
52) L499-501: This is for the karst area, how relevant is it for your area?

In our dataset we have two dolomitic catchments: BCC and OVA. We specify this in Table 1 and in Section 4.4 of the revised manuscript.

680
53) L518: I suppose the fast flow paths are due to the fact that the glacier acts as an impermeable layer and thus promotes rapid overland flow? Please explain what you mean.

This comment refers to a possible explanation of the high F_{yw} for the glacier-covered catchment. The “old” text reads as “Such (glacier-covered) catchments could show fast flow paths and small storages as e.g. discussed in the work of Jansson et al. (2003), reviewing glacier-dominated environments. Moreover, reduced baseflow during winter can be related to increasingly high temperatures causing the glaciers retreat, thus reducing, and anticipating the glacier melt fluxes that possibly recharge groundwater (Hayashi, 2020)”.

685
690
We have rewritten (see Section 4.4): “The high F^*_{yw} of the high elevation glacier-covered (42%) catchment can be explained considering that the glacier-melt produces high amounts of streamflow that transit the glacier-system very quickly during the summer, given generally fast englacial and subglacial flow paths and the often limited water storage capacity in the glacier forefield (Müller et al., 2022; Saberi et al., 2019; Jansson et al., 2003). Schmieder et al. (2019) also found a high young water fraction in an Austrian glacier-covered (35%) catchment leading them to the conclusion that the basin behaves like a ‘Teflon basin’ with fast transmitted ice melt, also if this behavior is differentiated in space.”

54) *Figure 13: Which subfigure is for lower altitudes (< 1500m) and which one is for higher altitudes? Figure caption: the word “panels” can be removed because I thought a panel always consists of two subfigures (e.g., the lower panel contains two subfigures c,d)*

695 **Subfigures titled with “Ephemeral snowpack” refer to lower elevations, while subfigures titled with “Seasonal snowpack” are for higher elevations. Please note that we have deeply modified Fig. 13 in the revised version according to Jana von Freyberg comments. See Fig. 15 of the revised manuscript.**

55) *L531: “unconsolidated sediments are not the only...” could be changed to “water storage in unconsolidated sediments are not the only ...”*

700

Thank you.

2.2 Response to Jana von Freyberg

Dear Jana von Freyberg,

705 **thank you for your care and attention during your reading of the manuscript, your positive remarks and your suggestions that helped to improve the work considerably. We have incorporated all your constructive feedback in the revised version of the manuscript.**

Please find below a point-by-point response to all your comments.

710 **With kind regards,**

The Authors

2.2.1 General comments

715 *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*

720 *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.

1) *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 make the research objectives and the novelty of our work clearer in the Abstract and in the Introduction.

For example, in the Abstract: “Past works have shown surprising evidence that mountainous catchments worldwide yield low F^*_{yw} . These low values have been partially explained by isolated hydrological processes, including deep vertical infiltration and long groundwater flow paths. However, a harmonious framework illustrating the relevant mechanisms leading to a low F^*_{yw} in mountainous catchments is missing.”

“The main aim of this paper is to give an overview of what drives the F^*_{yw} variations according to elevation, thus clarifying why it generally decreases at high elevation.”...

“As a conclusion, we develop a perceptual model that integrates all the results of our analysis to describe a framework for how hydrological processes control F^*_{yw} according to elevation, laying the foundations for an improvement of the theory-driven models.”

Please see the “1 Introduction” Section of the revised manuscript to observe all the relevant changes about the new formulation of the research objectives (linked to a specific research gap) and how we intend to fill this gap using some new (previously unconsidered) variables (F_{qd} , F_{bf} , LFD , F_{SCA}) reflecting some specific hydrological processes.

In the Introduction, we guide the reader to the importance of understanding such processes for explaining the changes of F^*_{yw} with elevation, mainly focusing on the reasons why such processes can be hidden behind the low F^*_{yw} at high elevations.

The fraction of Quaternary deposits, the low-flow Duration and the snowpack ephemerality have never been used to explain the low F^*_{yw} at high elevations. Thus, the relations between F^*_{yw} and such variables are novel. F_{bf} was indirectly used in past studies, but it was estimated with different methodologies. With the methodology presented in this paper, i.e., baseflow filter of Duncan (2019), we achieve a complementary relationship between F_{bf} and F^*_{yw} suggesting that $F^*_{yw} \simeq 1 - F_{bf}$. We can say that this is a by-product of our work (i.e., our main aim was not to find an alternative method to estimated F^*_{yw}). However, this is a novel

result since it suggests that F_{yw}^* could potentially be estimated for catchments in which stable water isotopes measurements are not available.

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Moreover, the research objectives are also clearly expressed in the new title: “Towards a conceptualization of the hydrological processes behind changes of young water fraction with elevation: a focus on mountainous alpine catchments”

765

- 2) 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.

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

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“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 the Staudinger et al. (2017) classification scheme cannot be strictly applied since they were designed for the Swiss hydro-climatic regimes. We have “manually calibrated” 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 catchments studied by Staudinger et al. (2017) and we can apply it also outside the Swiss borders.

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However, according to the referees’ comments, we have removed the new criterion for catchment classification from the revised version of the manuscript. We use the classification scheme of Stoelzle et al. (2020) which is based on catchments elevation, typical low-flow period, typical snow onset and typical begin of snowmelt. This scheme was used in the past by the authors to classify catchments outside Switzerland (i.e., German catchments).”

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- 3) 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?

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Thank you for this comment. We have clearly explained why we have not considered precipitation in the Introduction: “A special focus of our work is on variables that were not previously considered for explaining

elevation gradients of young water fractions. We namely exclude catchment size, annual precipitation, bedrock porosity, pasture cover, open water cover that have been discussed and shown to have little correlation in the work of Jasechko et al. (2016).

795 A special case in terms of explanatory variables is mean annual precipitation: Jasechko et al. (2016) in their worldwide study did not observe any significant correlation between the F^*_{yw} and annual precipitation. Lutz et al. (2018) found, based on 24 catchments in Germany, that F^*_{yw} decreases with increasing mean annual precipitation. In contrast, in the relatively wet rainfall-dominated and hybrid catchments studied by von Freyberg et al. (2018), F^*_{yw} was shown to increase with precipitation, which in turn both increase with
800 elevation. In their study, discharge (unsurprisingly correlated with precipitation) was considered as a proxy of catchment wetness, which favours rapid flow paths and thereby increases F^*_{yw} (von Freyberg et al., 2018). In snow-dominated systems, the use of mean annual precipitation as a proxy for catchment wetness could be misleading because the seasonal snowpack leads to a very dry period of the year despite the high *solid* water input. In other words, the temporal concentration of the liquid water input is the relevant variable. Indeed,
805 the saturation of the system (i.e., high wetness conditions) can be observed also when the annual precipitation is low if a large volume of water (stored in the snowpack) is released in a relatively concentrated time interval. Indeed, despite precipitations, and correspondingly discharges, are relatively higher in snow-dominated than in rainfall-dominated catchments, F^*_{yw} is generally lower in snow-dominated systems that are potentially wetter than rainfall-dominated ones. This suggests that the only precipitation can only
810 partially explain the variations of F^*_{yw} and that other variables should be put under observation.”

To be precise, in the Discussion we use precipitation to explain why F^*_{yw} increases with elevation until 1500 m a.s.l. (i.e., for rainfall-dominated and hybrid catchments), but we do not use precipitation for explaining why F^*_{yw} is low at higher elevations (i.e., for snow-dominated catchments).

4) *The important aspect of snow pack storage in high-elevation, snow-dominated catchments, which the authors only
815 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
820 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
825 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 very much for this comment. Following your suggestion, we have inserted a paragraph in the Introduction in which we clearly address the age of the snowmelt (we totally agree with you), the importance of snowpack storage and its role about the estimation of F_{yw}^* , specifying that it was previously addressed by von Freyberg et al. (2018). We have also introduced in this Section the difference between “direct” and “delayed” input and that, from a water storage perspective, and water age perspective, the snowpack and the groundwater storage can be considered as a single entity, thus they both constitute the catchment storage. This aspect was also deepened in Section “3.1 Young water fraction estimation from seasonal cycles of stable water isotopes in precipitation and streamwater: the “direct” input” of the revised version.

We would like to specify that in snow-dominated systems there is a thin line 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.

5) *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 revised paper (see Section 4.2) 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., 2022; Pavlovskii et al., 2018).

6) *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):*

7) *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 in the preprint we have not pointed out the fact that, in our analysis, we are assuming that the snowpack is part of the catchment storage. In the revised version this is

860 addressed firstly in the Introduction and, secondly, in Section “3.1 Young water fraction estimation from seasonal cycles of stable water isotopes in precipitation and streamwater: the “direct” input”. Accordingly, the F_{yw}^* of the revised version has been obtained through the “direct input” approach.

8) 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.

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Calculating the annual baseflow through another baseflow filter, e.g., the Lyne and Hollick (1979), the positive correlation between Annual baseflow and F_{SCA} does not change (please see Fig. 3a, Fig. 3b of the public ‘Reply on RC2’).

9) 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.

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We would like to underline that we do not pretend that snow cover persistence alone explains baseflow, we have simply shown the statistical link. However, we have decided to remove this sentence in the revised version.

10) 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.

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We made the link among snowpack storage, groundwater storage and catchment storage clearer in the revised version.

Snowpack releases old water during the melting season. Such meltwater preferentially infiltrates so that the groundwater storage is mainly composed by old snowmelt. This is why we can consider the snowpack and the groundwater storage as a single entity: the catchment storage. We quantify the contribution of the catchment storage (i.e., snowpack storage + groundwater storage) to the stream through the baseflow (which cannot distinguish between snowmelt and groundwater). Specifically, with the help of the baseflow, we can quantify the share of streamflow that is due to the catchment storage release. Of course, the share of snowmelt (with age > 3 months) that flows off quickly will not show up in the baseflow.

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In snow-dominated catchments, during the winter season, the baseflow measures the groundwater storage contribution to streamflow (but not the new snowpack contribution since such snowpack does not melt during winter). Such groundwater storage is mainly composed by the old snowmelt (previously infiltrated).

895 **The duration of the groundwater-sustained (or old water sustained) winter low-flow depends on the snowpack persistence in snow-dominated systems. The longer the persistence the longer is the contribution from the groundwater (or old water) storage to the stream. This is the reason why the low-flow Duration (LFD) explains the reduced F^*_{yw} in high-elevation catchments.**

These concepts and reasoning have been thoroughly addressed along the entire revised manuscript, especially in the “Results and Discussion” Section.

900 11) *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.”*

905 **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 have removed this part in the revised manuscript.**

910 12) *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?*

915 **We realized that Fig. 13 can be misleading since only a single catchment is represented. Therefore, we improve Fig. 13 to better reveal our “step forward” regarding the hydrological processes behind the F^*_{yw} variations between different catchments. Please see Fig.15 of the revised manuscript.**

2.2.2 Specific comments

920 1) *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...”

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Thank you for this comment. We report here the relevant changes made in the title described in Section 1.1 of this document:

“Starting from this comment we have decided to change the paper title:

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From: “What drives F_{yw} variations with elevation in Alpine catchments?”

To: “Towards a conceptualization of the hydrological processes behind changes of young water fraction with elevation: a focus on mountainous alpine catchments.”.

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We think that this title reflects much better the content of the paper, which is concluded with the presentation of a perceptual model (the first step before conceptualization) that integrates all the results of our analysis to describe a framework for how hydrological processes control the F_{yw}^* according to elevation. In the title, we underline that the focus of the paper is on mountainous alpine catchments. We use the term “alpine” instead of “Alpine” to refer to the typical hydro-climatic conditions of a mountain climate. In other words, we are not specifically referring to the Alps Mountain range. Finally, we have changed “ F_{yw} ” to “young water fraction” in the title as suggested by the referee #1: “... “ F_{yw} ” could be changed to “young water fraction” for general readers”.

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- 2) 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_{Jum}/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.

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We agree. In fact, F_{bf} and WFI have been calculated in the same time period of isotope sampling (called PoS in the revised manuscript): we specify this in the revised manuscript (Table 1 and Section “3.3 Fraction of Quaternary deposits, Low Flow duration and the groundwater contribution to the stream”). 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 specify this in the revised version (Section “3.2 Snow cover persistence quantified through the average fractional Snow Cover Area (F_{SCA})” of the revised version).

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For the Q_{June}/Q_{DJF} , we used a long-term average since this ratio was used for a classification purpose. However, we do not have more Q_{June}/Q_{DJF} in the revised manuscript since we remove our “new classification scheme” as suggested by the referees.

- 3) *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 have carefully reviewed the language of the revised version.

- 4) *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?*

We report here the answer we give to the minor comment n°31 of referee #1:

“We follow the approach of Hofmeister et al. (2022) and Di Marco et al. (2020) that define f_{SCA} as $N_{snow}/(N_{tot}-N_{clouds})$. They did not comment on the effect (i.e., underestimation or overestimation) of the mathematical expression of f_{SCA} on their results. If the two detection algorithms (snow detection and cloud detection) would work with a 100% accuracy, values greater than 1 cannot be encountered. In fact, the maximum cloud cover fraction can also be very close to 1 in some dates (e.g., > 90% as encountered in our data set), but if the snow detection algorithm works well N_{snow} will be at most “complementary” to N_{clouds} (i.e., $N_{snow} + N_{clouds} = N_{tot}$) and f_{SCA} will be at most 1. Sometimes these algorithms can result in a misclassification of pixels and f_{SCA} values > 1 can be encountered: i.e., $N_{snow} > (N_{tot}-N_{clouds})$. Our approach was to set $f_{SCA} = 1$ if $f_{SCA} > 1$. We deepen that this approach can overestimate the f_{SCA} . Thus, we propose a new approach: if $f_{SCA} > 1$ we calculate f_{SCA} as N_{snow}/N_{tot} since this is the only heuristic solution that guarantees no overestimation. Please see Section 3.2 of the revised version for more details.”

- 5) *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.*

We have explained the reason why we have used F_{SCA} in Section 4.4 of the revised version: “ F_{SCA} is strongly correlated with the mean catchment elevation in our data set ($\rho_{Spearman} = 0.97$, p-value < 0.01, Fig. 13b). A posteriori, we could have considered mean elevation instead of F_{SCA} as a proxy for snowpack persistence. However, a priori, 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 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 snowpack

persistence could considerably change. This is the reason why we decided to focus on F_{SCA} that integrates these physical factors.”

- 995 6) 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., 2020). Thus, for the other catchments, we consider negligible the effect of glaciers on F^*_{yw} (we explicitly say this in the “Study sites” Section of the revised manuscript).

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- 7) 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 $1/F_{bf}$ will likely not be exactly the same, although both were calculated through digital filtering of discharge time series).

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Thank you for this, we have cited in the discussion that a similar result was found by von Freyberg et al. (2018) (see Section 4.2 of the revised version). 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 $(1-QFI) \neq 1$). A good result of our work is that, using the recession constant 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., $F^*_{yw} + F_{bf} \approx 1$) 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 $1-F_{bf}$. We clearly explain the relation between F_{bf} and F^*_{yw} in Section 4.2 and Section 4.2.1 of the revised version.

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- 8) 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.

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Thank you for pointing out or language mistakes. Please note that we have removed the sentences about channels order because we do not want to generate confusion: we are studying F^*_{yw} changes with elevation among different catchments and not within a single catchment. Probably, considerations about the age of water flowing in higher/lower order channels is more suitable if looking for water age variations within a single catchment.

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- 9) 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 proposed in the preprint. However, using the classification scheme proposed by Stoelzle et al. (2020): “According to this classification scheme, the four Italian catchments (DOR, SOU, BCC and NBPV) are all categorized as snow-dominated (S). The classification of BCC is also consistent with the one given in a previous study without considering the application of a formal classification scheme (Penna et al., 2016).” We have written this in “Study sites” Section of the revised version.

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10) 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. All these information are reported in the revised Supplementary material.

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11) 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.

2.2.3 Technical comments

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

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

- 2) (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). Specifically, during a rain or snowmelt event, groundwater storage is generally recharged. This recharge is visible in the hydrographs reported in Fig. 8 of the revised manuscript through the “smoothed” baseflow, proposed by Duncan (2019). This “smoothing” simulates a delayed storage contribution to the stream following the recharge phase during an event.

- 3) (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?

Probably the term proxy is not correct. F_{bf} is the average fraction of baseflow and it is an estimate of the proportion of the catchment storage (snowpack storage + groundwater storage) contribution to the stream. The meaning of “roughly the one’s complement of F_{yw} ” is that, for each catchment, $F_{bf} + F_{yw}^* \approx 1$.

- 4) 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 have rewritten this part of the abstract in the revised version.

- 5) 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. We have modified the sentence in “The temporal dynamic of snow accumulation and melt supports the pivotal role of snowmelt in recharging groundwater during summer in high-elevation environments (Cochand et al., 2019; Du et al., 2019; Flerchinger et al., 1992).” See Section 4.4 of the revised manuscript.

- 6) 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 have used this notation in the revised manuscript.

- 7) 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.

1090 **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. You will find the right sentence in the revised Supplementary material.**

3 References

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