Dear Editor, Dear Reviewers

We would again like to thank you for handling our paper and giving additional advice. Below, we respond to the latest round of requests and mention the changes we've made or in some cases a justification for the lack of change. Our responses are introduced by double carrots >> after each reviewer comment.

Thanks to the reviewers' comments, the quality of the manuscript has further improved, and we believe that it is now ready for publication.

On behalf of all authors,

Natalie Ceperley

Reviewer 1

We are very thankful for the time and feedback you gave us. Please find below a point-by-point response to your specific comments.

Language. There are still typos, tense inconsistencies (switches from present to past in the same paragraph or even in the same sentence), some convoluted sentences. Please, have the language carefully revised by a native speaker or by a proficient speaker.
>> We have made some changes to awkward sections, in particular to the abstract and methods section.

2. The new title is not so convincing to me. It seems that the catchment is characterized by four tracers, and not the hydrodynamics. I suggest looking for a more attractive and informative title. For instance, I would mention something like "Interplay of hydrological processes in a high elevation Alpine catchment based on four tracers…"…or "Multiple tracers allow understanding of the interplay of hydrological processes etc."
>> After discussion between co-authors, we have decided to retain the focus on hydrodynamics and not further modify the title and keep it as:

> *Hydrodynamics of a high Alpine catchment characterized by four natural tracers.*

76. I suggest including here, or later, the specific research questions that the work addresses.
>> We think the last two paragraphs of the introduction do this already. Repeating the objective in the form of a question seems redundant.

179 and 361. Sorry, the use of the moving average is still not clear to me, and I am still afraid of the possible bias it could lead to. Please, clarify this point.
>> We've added more explanation that was in our reply to reviewers previously following the last round of revisions but not elaborated on in the paper:

> *"To guide the analysis of the streamflow response throughout the year, we analyzed the different streamflow periods in detail (Section 3.1) based on the 7-day moving average streamflow data (Qm7) and the daily change of Qm7, called ΔQm7. This moving average does not introduce a temporal shift since the seven-day window extends 3.5 days forward and 3.5 days backward, however it does reduce the peak*

flow. Its interpretation alongside the high-resolution streamflow data does not introduce a bias but rather highlights the slower processes."

952-953. Please, explain better (and perhaps include references) your point about sampling snow vs. snowmelt.
>>By rearranging the paragraph and combining it with the 4.4.4 O17 paragraph and adding more references, we believe this point is now clear:

"While so far not reported for measurements of $\delta^{17}O$, it is known that under certain environmental conditions, the snowpack will experience melting, evaporation, and refreezing, forming a firn rather than snow. This may influence the ^{17}O-excess measured for either fresh snow or the final snowpack (see, for example, Risi et al., 2010). Although, much laboratory time was devoted here to the measurement of $\delta^{17}O$ and ^{17}O-excess, we gained few insights or specific added value as we hoped for documenting the influence of local-scale snow dynamics, specifically the variation in space and time of accumulation, transport, storage, melt and sublimination, on hydrological processes, except some unvalidated potential to distinguish glacier melt from snowmelt when combined with temperature measurements. Perhaps our measurements would have been more relevant if fresh snow had been sampled instead of the snowpack or if more relevant reference data were available."

Subsections 4.4.3 and 4.4.4 are very short. I wonder if they could be merged or included in other subsections.
>> We merged the O17 paragraph above to the other isotope paragraph.

Conclusions: I'd avoid the over-use of the term "reset".
>> Thank you, we've reworded or elaborated on all the mentions of "reset" in the conclusion.

Minor comments and technical corrections
17. Store = storages?
>> We changed this in the abstract to:

"Although diverse water sources and flow paths generate streamflow in the world's "water towers" emerge from these two driving inputs, a detailed process understanding remains poor."

186. Although this is quite common, this is not the correct terminology. I suggest using stable isotopes "in" water, or "of oxygen and hydrogen".
>> We've modified this expression throughout the paper:

"Stable isotope composition of water"

217. "von", with no capital letter.
>> Thank you – we've made it lower case.

256. Remove "familiar".
>> Thank you .

318-319. Include references, please.
>> We've added more explanation and references:

> *"Although it is common to study the effect of snow cover on near surface soil temperature especially for prediction of microclimates and habitats (Rixen et al., 2008; Freppaz et al., 2018; Giaccone et al., 2019), the inverse focus is also relevant and soil temperature is a good proxy for snow cover (Bender et al., 2020; Staub et al., 2015), making distributed observations of soil temperature particularly useful for observing the expansion and contraction of snow cover."*

721. Comparison of what? Please, specify.
>> We've reworded this for more clarity:

> *"Comparison of similar tracer studies in Alpine regions"*

726. What is the headwater status? Please, clarify.
>> We've reworded this for more clarity:

> *"The slightly lower values can be explained by the proportion of snow to rain in the specific headwater region of this study relative to other regions."*

797. What do the Authors mean by "prevalent"? Please, explain.
>> We've reworded this for clarity:

> *"The early melt period is rarely discussed in the literature (for a model-based example, see He et al., 2015), despite it being its importance in terms of water supplied to the catchment and prevalence across Alpine regions, and the streamflow during this period remains challenging to model (see Figure 9 in Brauchli et al., 2017; or Figure 3 in Thornton et al., 2022)."*

851. Give reference for the old water paradox.
>> We've added references:

> *"Our measurements clearly suggest that this so-called "old water paradox" does not hold for some rainfall-generated streamflow responses during the recession period (Mcdonnell, 1990; Mcdonnell et al., 2010)."*

Reviewer 2

In my review of the initial submission, I raised my main concern that the study of Michelon and colleagues was a bit descriptive and could benefit from a modeling component. Partly in response to this - but I cannot be sure - the authors added a conceptual drawing about the succession of streamflow generation periods at their experimental site. This is a first step, however the contrast between the amount and quality of the data and the qualitative interpretation remains. The authors argue elsewhere that their isotope data (more precisely their dynamics) are too complex for a straightforward interpretation. I agree with this statement but in those situations a model application could certainly shed light.
Since my opinion does not seem to be shared by the other reviewers and that the data alone is worthy of publication (for others to model them), I will personally not object to its publication.

>> Thank you for your recommendation. While we agree that a modelling approach would help improve our quantitative interpretation of the phenomenon, we believe this is outside the scope of the current paper. We will add a sentence in the future outlook that emphasizes your recommendation:

"A future investigation in this location could render our conclusions more quantitative by using a modelling approach to explore our observations."

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