

Hydrol. Earth Syst. Sci. Discuss., author comment AC1
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Reply on RC1

Anthony Michelon et al.

Author comment on "Hydrodynamics of a high Alpine catchment characterized by four natural tracers" by Anthony Michelon et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-48-AC1>, 2022

Dear first reviewer,

We are very thankful for the time and feedback you gave us.

Please find below a point by point response (following double carrots >>) to your specific comments. In addition to general clarifications, we have 1) proposed a first conceptual figure; 2) performed an analysis of moving average effects; 3) explored how the streamflow periods B, E, and M relate to the stream flow percentiles. The clarifications could be transferred to the manuscript in a more condensed manner, which we would be happy to implement. We will carefully revise the manuscript and incorporate your minor comments and technical corrections once we receive the editor's response.

Sincerely,

The Authors

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- The abstract is too generic and vague. I suggest rewriting it and make it more process-oriented so the reader can better appreciate what are the main findings and contribution of this work in terms of understanding the hydrological functioning of this catchment.

>> We will be happy to rework the abstract to focus on the process oriented process and highlight the main findings.

- Introduction:

1) The first part of the Introduction works well but what is missing is a clear statement about the main gaps in the current literature and the main research gaps that this work aims at addressing. Reporting that "this work attempts to quantify dominant drivers..." is ok for a general objective but this is not clearly link to a current lack of knowledge that this work could contribute to fill in. Please, rewrite lines 51-63.

>> We will rewrite lines 51-63 with a more clear statement, such as:

To fill the gaps in existing studies, namely the elevational and the seasonal gaps, this

work quantifies drivers of the hydrologic response of a high elevation catchment throughout all streamflow periods, ranging from winter low flow, to different stages of the melt season and the autumn recession by compiling and complementing observational data from the intensively studied Vallon de Nant catchment in the Swiss Alps (Benoit et al., 2018; Giaccone et al., 2019; Ceperley et al., 2020; Mächler et al., 2021; Michelon et al., 2021a; Thornton et al., 2021a; Beria et al., In revision; Antoniazza et al., Submitted).

This work brings together four different tracers in a unique fashion. First, stable isotope composition of water, a natural tracer that has been extensively used to characterize hydrological processes related to snow (e.g. Beria et al., 2018), which is particularly useful to examine the interplay of different water compartments (rainfall, snowpack, springs, groundwater), recharge and evaporation processes (e.g. Sprenger et al., 2016). Second, electrical conductivity measurements that provide additional information on subsurface flow paths and relative water residence times in the subsurface (Cano-Paoli et al., 2019). Third temperature measurements of water which trace connectivity between water sources and the atmosphere (Constantz, 2008). And fourth, soil temperature measurements which inform identification of periods of thermal insulation from the seasonal snowcover (Trask et al., 2020).

2) Similar, the specific objectives (64-70) are all lumped together, and this does not make justice to the worked carried out. I strongly suggest restating them as specific questions that can be measurable and are clearly linked to the different sub-sections of the results.

>> We will rewrite lines 64-70 with a more specific questions, such as:

The specific objective of this work is to examine the dominant hydrological processes that explain the catchment-scale hydrological response during different periods of the year through the lenses of four different tracers: soil temperature, water temperature, electrical conductivity, and stable isotopes of water. Using these four tracers, we shed light on 6 questions. 1) What is the origin of winter streamflow (from subsurface storage versus from localized snow melting) (Floriantic et al., 2018; Hayashi, 2020); What are the dominant runoff processes that 2) drive streamflow generation during early spring snow melt (Brauchli et al., 2017)?,3) later on in the snowmelt season?, and 4) during the season recession? 5) What is the role of shallow groundwater in the hillslopes and of alluvial or talus groundwater systems (Hayashi, 2020) in the streamflow generation throughout the year. Finally, what transferable insights into the value of these four tracers for hydrologic process investigation are relevant for comparable catchments?

3) The weakest part of this manuscript is the Discussion:

A) it's highly fragmented in small subsections that, instead of making the structure clearer, produce a confusing picture.

>> The discussion actually mirrors our specific questions exactly, but to make that more clear, we will redefine the structure according to the following outline, which the 3 sections of dominant runoff processes grouped together as subsections:

- 5.1 origin of winter streamflow
- 5. 2 dominant runoff processes
 - 5.2.1 during early spring snow melt
 - 5.2.2 during melt periods
 - 5.2.3 during the seasonal recession
- 5.3 interplay of shallow groundwater in talus systems

- 5.4 Instead of presenting the last section, “lessons learned from our four tracers” (previously: Transferable insights into the value of the observed variables for hydrologic process investigations in comparable catchments), as a single section with subsections, we will break it into a final paragraph at the end of each other section of the discussion and a summary paragraph that will go in the conclusion.

B) The results are discussed “internally” and there are almost no references to other works. This make it confusing to the readers to understand what is different and new compared to previous knowledge, and what is the contribution of this research.

>> Thank you for your point of view and pointers of key references, we will happily develop the discussion further along these lines.

C) As a results, the novelty and the original contribution of this paper (which, in my opinion, exists) is hidden and not stressed at all, making it, unfairly, a general case study.

>> As we revise this manuscript, we will emphasize the unique contribution of this study to hopefully move it out of the general case study category.

D) As a results of the confusing structure of this Section (point 1), the most important finding of this work is not very clear to the reader as it could be, ie, how this catchment responds to precipitation and snowmelt inputs! Time series and reported ranges of values are of course important but I suggest adding a sort of conceptual graph showing the main runoff generation processes and the general behaviour of this catchment (a sort of graphical conceptual model of catchment response). I think this will make the paper of more impact.

>>In the attached supplement is the beginning of a conceptual figure (Fig. 1) that we plan to integrate in the final manuscript. We will continue to improve, discuss, and integrate it as we prepare the final manuscript.

4) Finally, the language is overall good but sometimes there are not accurate terms, vague descriptions, and awkward sentences. Since there is at least one native speaker (I assume) in the authorship and other well-known and highly-experienced researchers, I believe that the language issues could be easily fixed.

>> We will happily work through the whole manuscript with this in mind.

5) At line 248 a 7-day moving average is mentioned and applied. In my experience, moving averages of such a long period produce i) a slight temporal shift of streamflow peaks; ii) a reduction of streamflow peaks. Please, comment this issue and its possible effect on the results, or adopt another smoothing method.

>> This is a good point. In this case we are just using the moving average to have a better image of what is going on and help determined the stream flow periods.

i) There will not be a temporal shift since the seven day window extends 3.5 days forward and 3.5 days backward. Please see the attached figure (Fig. 2 in the attached supplement). ii) There is a reduction of the peaks, which helps us to interpret the processes. This information is used along side the actual data not instead of it. We will make our method and purpose more clear in the text.

6) At line 249 the Authors introduce the three streamflow periods B, E and M. Although I overall agree with them I’m afraid that this is an arbitrary selection not based on an objective criterion (eg, the use of a streamflow percentile threshold). Please, comment this issue and its possible effect on the results, or adopt another more objective selection

method.

>> We have compared our periods, and come up with some more quantitative guides, visible in a table (Table 1, attached supplement). A condensed form of this table and points could be added to the manuscript or the supplementary material. The divisions were identified as follows:

- The transition from M to R is always the peak of the 7 day moving average.
- Flow is increasing in E and M and decreasing in R and B.
- E is the first "wave of discharge", where the flow increases but the daily range doesn't yet jump up.
- During M, there is a much higher diel variation in flow.
- Similarly from R to B, the daily variation drops off.
- B has median of daily flow quantiles < 30, E around 50, M > 80, and R around 70 or 75
- The change in flow of B per day (using 7-day average data to override event effects) in m³/s/d is around 0, E around 0.1, M >0.1 up to 0.3 and for R is <0 around -0.1
- The median peak time of discharge is not as consistent, but it is late at night for E and closer to mid day or late afternoon for M. During R, it might be early afternoon or morning. B is not at all fixed
- The range of daily values is low, around 0.02 in m³/s for B, 0.1 for E or R, and high 0.3 for M.

7) 523-551. I suggest combining subsections 5.2, 5.3 and 5.4. I also suggest combining 5.6.3 with 5.1.

>> Thank you for this suggestion, it fits with our plan to restructure the discussion in point 3A.

8) Why can it be a calibration issue? What is this statement based on? Please, explain.

>> Their divergence is unexpected. However at the end of base flow, it is very likely that there was not adequate water in the piezometers for accurate measurement. We were referring to this phenomenon. We will change our wording in the manuscript.

Please also note the supplement to this comment:

<https://hess.copernicus.org/preprints/hess-2022-48/hess-2022-48-AC1-supplement.pdf>