

Hydrol. Earth Syst. Sci. Discuss., referee comment RC1
<https://doi.org/10.5194/hess-2022-48-RC1>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on hess-2022-48

Anonymous Referee #1

Referee comment on "Studying the dynamic of a high alpine catchment based on multiple natural tracers" by Anthony Michelon et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2022-48-RC1>, 2022

Review of the manuscript 'Studying the dynamic of a high alpine catchment based on multiple

natural tracers' (hess-2022-48)

General comment

This is a very interesting manuscript dealing with the analysis of hydrological processes and streamflow generation mechanisms in a high-elevation Alpine catchment. The study leverages on a wealth of data collected in the well-known Vallon de Nant catchment in Switzerland taken as an example of snowmelt-dominated Alpine catchments, and from which results could be generalized. The manuscript aims at disentangling the complex dynamics of runoff production in these environments where multiple water sources and landscape topographic feature contribute to make the hydrological response quite complex.

Overall, the manuscript is well organized, results are sound and well supported by the data (although there is a methodological issue, see specific points below), and graphs well prepared. However, in my opinion, there are some major issues that requires a throughout revision.

Specific comments

- 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.

- 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.

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.

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.

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.

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.

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.

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.

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.

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.

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

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

Minor comments and technical corrections

15. Although common, "stable isotopes of water" is not a correct terminology as isotopes are of oxygen and hydrogen, not water. Please correct here and elsewhere (eg, 151).

27. The language needs to be revised.

Fig. 1. I suggest removing the catchment name from the figure (my personal taste, of course).

193. A couple of references that could be cited here on memory effect are the following:

1. DOI: 10.1002/rcm.7824
2. <https://doi.org/10.5194/hess-16-3925-2012>

215-216. I suggest moving this part before introducing the Ic-excess.

268. There is an extra "s".

286-288. This is quite typical in Alpine environments...I suggest skipping all this part.

Fig. 3. The legend is not self-explanatory, please add a specification in the caption. Also remind about the meaning of B, E, M and R.

311-315. Move to Discussion.

321. I suggest replacing "personal" with "field".

339. The language needs to be revised.

353. Perhaps "approximate" is more appropriate than "rough".

379-383. Move to Discussion.

438-439. The language needs to be revised.

445-446. The language needs to be revised.

458-460. Move to Discussion but explain what this statement is based on.

466. Some readers might not be aware of the relation between d-excess and the GMWL. Please, shortly introduce it.

471. Where is this reported? This is not clear.

478. Please, report the observed differences.
485-486. The language needs to be revised (can water be recharged? A reservoir/compartiment can).

488. The language needs to be revised (have/has).

500. Which Figure?

517. Give a reference.

535. Low or high number?

541. Why "however"?

559. Include a reference.

580. Which Figure?

590 Water sources to what? Explain.

Suggested literature

Here is a short list of papers that in my opinion could become useful in the Discussion

1. <https://doi.org/10.1016/j.jhydrol.2019.123983>

2. <https://doi.org/10.1016/j.jhydrol.2021.127209>

3. <https://doi.org/10.1016/j.jhydrol.2021.125983>

4. <https://doi.org/10.5194/hess-21-23-2017>

5. <https://doi.org/10.5194/hess-23-2983-2019>

6. <https://doi.org/10.1016/j.jhydrol.2021.126437>