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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Monitoring snowpack outflow volumes and their isotopic composition to better understand streamflow generation during rain-on-snow events" *by* Andrea Rücker et al.

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General comment

First of all, I apologize with the Authors and Editor for my late review.

This is a very interesting manuscript that focuses on the role played by rain-on-snow (ROS) events in enhancing snowpack outflow and thus snowmelt, ultimately contributing to stream runoff. I worked for some years in a snow-dominated catchment and I had the opportunity to observe the significant impact that ROS events have on the catchment hydrological response in the melting period. Therefore, experimental work that

provides a better understanding of the controls on snowmelt contribution to streamflow during ROS events is welcome and certainly appreciated by the readers of HESS.

The manuscript is well written, solidly structured, nicely illustrated, with updated and relevant references, and the data well support the results interpretation. I basically agree with the comments by the two other Reviewers and I overall like the response of the Authors. I have only a few specific comments that I hope can contribute to improving the manuscript. In the end, I recommend a minor revision before publication.

Specific comments

- In agreement with Reviewer 1, I also noticed the lack of a clear and testable research hypothesis stemming from the knowledge gaps defined earlier in the Introduction. The Authors replied that the main hypothesis "...is that vegetation and elevation substantially affect the generation and the isotopic composition of snowpack outflow, and thus snowmelt contribution to streamflow. In my opinion, this reply is not fully satisfactorily. First, "vegetation" is quite a vague term in this context: reading the rest of the manuscript and knowing the area it is clear that this term refers to forest trees but, in principle, this could be valid for understory vegetation as well. So, I suggest being more specific here. Secondly, what does it mean that vegetation and elevation affect snowpack outflow generation? I guess the Authors mean outflow amount or volumes, but again this should be specified. Most importantly, this is only the general hypothesis. I suggest to complement it with some specific hypotheses or specific research questions that better address the core of this work and around which the Results and Discussion section could be built. For instance, one specific research question could focus on the role of rainfall characteristics and initial snowpack properties on the variability of snowpack outflow volume. Another specific research question could deal with the spatial variability of snowmelt contribution to streamflow in the catchment (comparison of hydrograph separation results among the three sites) and a third one to the temporal variability of snowmelt contribution to streamflow (comparison of hydrograph separation results among different ROS events). These are only suggestions but I think

that structuring the Results section so that its parts reflect the specific questions posed at the beginning would tell a clearer story and accompany the reader in a more linear way.

- I think that what would be really interesting and novel is the application of three-component hydrograph separation to quantify the proportion of rainfall and pre-event snowmelt during ROS events. As far as I understood, the instrumental design and the sampling scheme would allow for the application of this mixing model that, of course, requires the availability of a second tracer. Is there any additional tracer available? Is the application feasible? Are there theoretical or practical constraints that prevent this analysis? I wonder if the Authors already planned a follow up of this study considering this aspect. A comment on this is welcome.

- At lines 268 and 269 the Authors stated that the pre-event tracer signature (by the way, talking about isotopes I think that the terms “signature” and “composition” are more appropriate than “concentration”) was determined by sampling the stream on the day prior the ROS event. In a previous study in a snowmelt-dominated Alpine catchment (Penna et al., 2016, JoH, https://doi.org/10.1016/j.jhydrol.2016.03.040), we compared two different methods to determine the pre-event stream signature for two-component hydrograph separation during snowmelt, ie the average of several samples taken during baseflow and a sample taken before the snowmelt-induced runoff event. We found, in some cases, marked differences in the estimated snowmelt proportions in the stream using the two methods, and we related these differences to the fact that streamflow may have contained a small amount of residual snowmelt water at night, especially late in the melt season, so that meltwater influenced the isotopic composition of the stream between melt events. In the case presented by the Authors, the ROS events occurred in winter (Jan-March) and so this effect might not be so important but, nevertheless, I wonder if this effect could happen here as well at least in the late winter events (eg, March). A comment on this could be useful.

Minor comments and technical corrections L126. Remove “at mid elevations”.

L147-148. I suggest shortening the title.

L179: What is the relative measurement uncertainty of the tipping bucket?

L223. Remove the delta sign, it’s not needed here.

L255. Replace “concentration” with “composition”.

L259-260. Did the Authors/technicians apply any procedure to mitigate the carry over (memory) effect that can affect laser isotopic measurements when analysing subsequent samples with much different isotopic composition?

Fig. 3b: Add “rainfall” before the word “retention” below the 1.1 line.

L445-446. I suggest skipping this, redundant with what previously mentioned in the M&M section.

L456. Is the regression statistically significant? Can the Authors report the p-value of this regression?

L540-542. This sentence is not necessary and can be skipped.

Fig. 6. It is not immediately clear to me which rain samples are, which snowmelt samples, and ROS samples and bulk snow, so I suggest making the box plot clearer. In addition, did the Authors perform a statistical analysis in order to check for the differences in isotopic composition? Moreover, I wonder if the slope of the regression lines in the dual isotope space (Fig. 6d-f) is statistically different between the MG site and the HG and MF sites (see, for examples, another isotopic study on rain and snowmelt in the Alpine catchment mentioned above, Penna et al., 2017, HP, https://doi.org/10.1002/hyp.11050). This could be performed and discussed in the light of the inter-site comparison in rain and snowmelt isotopic composition.

L658. Replace “concentration” with “composition”.

L706-707. Which assumptions were violated to have unrealistic results?

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